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VOL. XXVI

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are J. Bostock, M. D.; Mr. J. Cuthbertson; A. M.; J. A. De Luc, Esq., F. R. S., &c.; Mr. Charles Sylvester; J. D. Maycock, Esq.; P.; Richard Lovell Edgeworth, Esq. M. R. I. A.; Prof. Wood; Luke Howard, Esq.; Mr. G. J. Singer; T. Le Gay Brewerton, Esq.; G. O. P.

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The Engravings consist of 1. Mr. Cavendish's Method of Dividing Astronomical Instruments; 2 Professor Copland's insulating Handle for an Electrical Machine; 3. Mr. Robert Salmon's Man-trap for securing Depredators without injuring them; 4. his Screw for fixing any Thing securely in the Ground; 5. his Method of building Pisé, or Earthen Walls; 6 Mr. De Luc's Apparatus for Analysing the Galvanic Pile; 7. his Dissection of the Galvanic Pile 8. Mr. George Prior's Clock Escapement; 9. Mr. G. D. Ross's Eye Bath; 10. Diagram to illustrate Prof. Wood's new Theory of the Rotary Motion of the Earth; 11. Mr. G. Williams's Method of Securing the Beams of Ships, without wooden Knees; 12. Mr. J. Varty's improved Linchpins; 13. An Iron Cylinder burst by Electricity; 14. Arrangement of the Strata of the Hill of Durbuy; 15. Mr. Mason's Trochar for the Relief of Hoven Cattle; 16. Mr. Fisher's Swivel-headed Churnstaff; 17. Mr. Tansley's Shag Cutter; 18. Mr. Davy's new Electrochemical Apparatus.

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MAY, 1810.

ARTICLE I.

Remarks upon Meteorology. *By* J. Bostock, *M. D.*

(Concluded from Vol. XXV, p. 208.)

To Mr. NICHOLSON.

SIR,

I Now resume my remarks upon the weather in September last, in doing which I shall have occasion to bring forwards some of my peculiar ideas on the subject of meteorology. I shall, however, as much as possible avoid entering into any hypothetical discussions, my present object being merely to illustrate my method of making observations. Weather in September last.

The beautiful evening of the 1st was succeeded, as I expected from its transparency, by a cloudy morning and falling barometer; rain came on about noon, and continued for two hours, and in the night there was a considerable squall of wind and rain. In the afternoon I have noticed a particular formation of the clouds, which I call *an arc*; it consists of a body of clouds, stretching in nearly parallel lines over a considerable part of the heavens, and converging in Form of cloud called an arc.

in the horizon; sometimes the lines converge at in opposite parts of the horizon, while at other lines shoot up to the zenith and terminate there. the shape, extent, and position of these arcs, and upon their relation to the wind and the barometer, many meteorological predictions depend. In the first place as to their shape, they may be composed of long parallel lines or threads, forming *a linear arc;* or of small rounded clouds, lying side by side or in rows, *a mottled arc;* or they may be composed of clouds resembling a volume of smoke, as it rises from a chimney top, *a wreathed arc;* or they may assume the appearance of feathers, having a linear centre and lateral branches, *a feathered arc.* Then independent of any particular shape, the arc may be perfectly or imperfectly formed; it may reach only to the zenith, or it may reach quite across to the opposite point; it may be either increasing or diminishing in size, forming *a precipitating or dissolving arc;* or, according as it coincides with the present, future, or past state of the wind, it will take the title of *a present, future, or past arc.* *A present arc* almost always indicates, that the wind will leave the point from which it is blowing; and when it arises from a S or W quarter, we may form a pretty good judgment whether the wind will go to the right or left hand, by noticing whether the barometer be rising or falling; if rising, the wind will pass to the right hand, if falling, to the left; in employing the terms right and left, I suppose the face of the observer turned to the point of the horizon from which the arc proceeds. The appearance that was observed on this day was *a future arc,* and according to a pretty general rule, that whenever an arc is formed to the left hand of the wind, and the barometer at the same time falls, the wind will move into that point, we had in the evening a squall from E. On the 4th was another arc, which passed from S to N; an expression which signifies, that it had two converging points in opposite parts of the horizon, and that it was most distinct in the S end; had it been equally perfect at both extremities, it would have been expressed, an arc between N and S. As the wind and the arc crossed each other at right angles, the direction of the arc did not inform us, whether the wind

Indications of the wind from it.

would

would move to the right or the left; but there were several circumstances, which rendered it probable, that the wind would go towards S; the arc was strongly precipitating and there was a small *contiguous solar halo*, by which is meant, a halo that is close to the sun, and not one that forms a large circle at some distance from it. This combination of phenomena seemed to show, that there was a current of air passing above the wind in a southerly direction, and that this upper current was loaded with aqueous particles in a different state from those in the wind. When I first observed these appearances, I fully expected rain from the S; but when, after some time, I perceived, that, although the rapidity of the upper current increased, yet the lower one decreased and tended to a different direction, I concluded, that the commotion, which at that time affected the upper regions of the atmosphere, would not be felt in this district. Accordingly, only a few drops of rain fell, and for the next 36 hours there was nothing which could be called a shower. This is one of those cases in which comparative observations made in different countries would be extremely interesting; we might, by their means, trace the exact limits of a storm, and probably be enabled to ascertain the causes which immediately produced it.

Halo.

Comparative observations.

The 5th was what I call *a revolving day*, i. e. where the wind gradually moves round through the different points of the compass; a change which must be distinguished from that where it ceases in one quarter, and then springs up in a different one. The revolutions are either *direct* or *reverse*, i. e. in the same direction with the course of the sun, or contrary to it. A reverse revolution, such as took place on this day, is a very general sign of rain, whereas a direct revolution is what often takes place in the most settled state of the atmosphere; a rainy night succeeded. During this time there were many indications of the atmospherical electricity being in what is usually called a negative state, which generally takes place when the wind is E with a low and falling barometer, in opposition to the more usual kinds of E winds, which are accompanied by a high barometer, and a strong positive electricity. In cold weather this combination of circumstances is generally attended or succeeded by

A revolving day.

Indications from it.

Atmospherical electricity negative.

the heaviest falls of rain, and in summer, by a close, oppressive state of the air, and thunder. The 7th was also *a negative day*, the barometer fell still lower, there were large thunder clouds at noon, and lightning in the evening.

Indications. A different constitution of the atmosphere, however, now took place, it became dissolving, the barometer began to rise, and continued to rise with occasional interruptions for several days. This change of constitution, the commencement of a rise in the barometer, and the arc of the following morning, rendered it probable, that a complete change of wind was at hand, and it accordingly took place in the afternoon. The 9th was a different kind of day from any that we had had since the beginning of the month; I denominate it a sour day, a phrase employed by landscape painters, expressive of that state of the atmosphere, when the whole face of nature appears chilled and gloomy. The NW current was hastily filling up the partial vacuum that had taken place during the low state of the barometer, and being deficient in caloric, it produced that large body of clouds, which gave the peculiar character to the day. By the term *shaded clouds* I mean to designate the appearance which the heavens occasionally present, where the clouds are formed into rounded masses of greater or less extent, one side of which is very much darker than the other side, in the same manner that a solid globe is affected by the light shining upon it. By *piled clouds* is meant that state in which a part of the horizon is occupied by a quantity of large rounded clouds, which appear as if they were heaped one upon another.

Sour day.

Shaded clouds.

Piled clouds.

Indications of unsettled weather.

The barometer had now attained nearly its average elevation, but the coldness of the air, and the peculiar appearance of the clouds, pretty certainly indicated, that the atmosphere was not in a settled state. If, after the influx of a cold current from the NW, the temperature of which is too low to keep its water in solution, the wind passes into a southerly point, rain almost always ensues, and the morning of the 10th exhibited a good specimen of the manner in which this operation is performed. In the early part it was bright, clear, and calm; but clouds soon began to form, which rapidly increased in bulk, and after some time were

no longer able to support themselves. The lines which were observed passing from the W were, however, a proof, that there was still an upper current in this direction; and it was probable, from the little loss of weight which the atmosphere had experienced, that this current would finally prevail. The change took place in the afternoon, when the atmosphere exhibited an appearance of greater tranquillity than it had done for many days. For a few hours there appeared nothing to disturb the equilibrium, but the formation of a wreath of clouds, composing an arc from NW, indicated a current of air passing from that quarter, which, by its greater density, would probably rush into our region, and produce a new constitution. The state of the next thirty-six hours might be considered as the immediate effect of this NW current, although with us it assumed a westerly direction. There was a cold breeze, large clouds were flying along, which were dense and disposed to dissolve, evaporation went on rapidly *, and I was much inclined to predict a favourable state of the weather, when on the evening of the 12th, the distant mountains were observed to be transparent, affording a pretty certain indication, that the wind would change to a precipitating point, and of course a strong presumption of rain. The following day was accordingly precipitating in a high degree; the atmosphere rapidly deposited its water, and the air rushed in to supply the partial vacuum that was thus produced.

On the 14th a constitution of the atmosphere took place, for which I was not prepared; a highly dissolving current of air proceeded from the E. As is the case with a *positive* E wind, the air soon became clear, and the weight of the atmosphere considerably augmented, but, according to a popular observation, which I believe to be very generally correct, a continuance of this state was not to be expected, both from the circumstance of its coming on so hastily, and also from its having been so immediately preceded by a S wind. The *tufts* that were observed, particularly those after the change, confirmed this opinion, for as they proba-

Positive E wind.

Tufts.

* I have heard it remarked by old people, that, when evaporation goes on rapidly, the streets drying quickly after rain, more rain will soon follow; and I have frequently found it so. C.

bly

bly depend upon partial currents of air, possessed of different properties from the prevailing one, so they lead us to suspect, that the mixture of these will produce some change in the equilibrium of the atmosphere. The appearances to which I give the name of *tufts*, are those clouds which resemble bunches of hair, the fibres of which are sometimes disposed in a perfectly irregular manner, and at other times lie nearly in a parallel direction. I believe, that, when these tufts point to any quarter, there is a current passing from that quarter, but there are many circumstances to be taken into account, before we can conclude that this partial current will become the prevailing one.

Indications of change.

The 15th, although calm and not unpleasant, had not that kind of calmness, which denotes a settled state of the atmosphere; but rather indicated the approach of some change in its constitution. The veering of the wind may be considered as an almost certain precursor of a change to a precipitating point, and a consequent diminution of the weight of the atmosphere. The appearance of the clouds rendered it probable, that there were at least two currents then prevailing, one from the NNW, and one from the E, while the appearance of the flocks led us to conclude, that the E current terminated in a stratum of air that was strongly precipitating. *Flocks* is a popular term, sufficiently expressive of a particular appearance of the clouds, when they form larger and more compact masses than those which I have called *tufts*; the fibres of which flocks are composed are also more generally parallel to each other than those of tufts.

Flocks.

The weather of the 16th, 17th, and 18th, may be considered as the result of that state of the atmosphere which was forming on the 15th; the barometer sunk considerably, the wind was in the S or W points, and sometimes high, with frequent rain, until on the evening of the 18th it proceeded to a violent storm. The atmosphere had lost three-quarters of an inch of its weight, and on the morning of the 18th it rained for several hours with a gentle breeze from SSE, at the same time that the clouds were moving rapidly from that quarter. Hence we might conjecture, either that there had been a partial storm to the SE, the effects of

Storm.

which

which would not reach us, or that to the N W there was a decomposition of some part of the atmosphere, commencing in the higher regions, to which the upper current, in which these clouds moved, was rushing. The appearances that afterwards took place proved, that this latter supposition was correct; for, although the wind went to the W, the diminution of the weight of the atmosphere still continued, while the appearance which I have described, of white clouds on a gray ground, is one of the most certain indications of the mixture of two currents possessed of different constitutions. It is to be observed, that, before the storm attained its greatest violence, the barometer began to rise; and as it depended merely upon the tendency of the air to produce an equilibrium of pressure, it was probable, that it would neither be of long continuance, nor very extensive.

White clouds on a gray ground.

I am here led to notice the difference between this kind of storm, which is produced by an abstraction of part of the atmosphere, and that which appears to originate from a contrary cause, from a sudden increase of the volume of the atmosphere. Of this latter species a well marked instance occurs while I am writing this paper. Yesterday, March 25th, we had a strong gale from the E; the barometer had been before about the medium height, but it rose during the continuance of the storm; to day the wind is more moderate, and the atmosphere is becoming lighter. I may here observe how much insight would be gained with respect to the theory of the weather, had we a number of accurate comparative observations made in different places on the progress of such storms as the one that took place on the 18th of September. Were we to ascertain exactly at what hour the barometer got to its minimum, when it began to rise, how long after this storm acquired its greatest violence, when it began to abate, and when the barometer arrived at its maximum, we should probably have gone a great way towards obtaining a correct theory on the subject. One important point might be ascertained, whether a storm be retrograde or progressive in its formation. I apprehend, that, where the current is rushing forwards to supply a vacuum, the storm will be retrograde; i. e. supposing the partial vacuum to have taken place over the eastern part of the

Storms from diminution and increase of the atmosphere.

Comparative observations

county

county of Lancaster, the storm, if proceeding from the W, will first be felt in this place, then it will recede to the coast of Wales, then to Anglesea, the Irish Channel, &c., until the equilibrium be established. But in the storm of yesterday probably the contrary course would be observed; the current being formed by an absolute increase of the volume of the atmosphere, it will push forwards like a current in any other kind of fluid, and will abate in proportion as the addition to the bulk of the atmosphere abates. If we pursue this idea so far as to suppose, that storms from a W point originate from the first, and those from an E point from the second of these causes, we may perceive why the former are more sudden in their commencement, are preceded by greater indications of commotion in the atmosphere, are attended with greater variations of the barometer, are more squally in their progress, more partial in their extent, and generally of less duration.

I shall not think it necessary to proceed any farther with the remarks upon my diary, as I conceive, that I have given enough to afford a complete illustration of my ideas on the subject. It is evident, that the first object is to obtain a full historical description of the successive changes of the atmosphere, to notice the periods of their commencement and duration, and the connection which they have one to the other. The second great point is to invent some method by which these changes may be accurately recorded; and it was here that I experienced the greatest difficulty, and that my diary will probably be regarded as most imperfect. The terms that I have employed will be thought very uncouth; and I have not unfrequently endeavoured to improve them, by forming a nomenclature upon scientific principles. I however finally determined to postpone the attempt, as I found it impossible to construct a nomenclature without involving in it more or less of hypothesis; and although it is difficult for any one to have thought so much as I have done upon the subject of meteorology, without forming an hypothesis, yet I do not consider mine as at present sufficiently mature to meet the public eye. It appeared therefore more advisable, not to attempt any thing like a methodical nomenclature, until there was a probability of its being founded upon well established

Objects in meteorology.

Terms.

blished principles. Not only is it desirable to avoid frequent changes, but from the influence which language exercises over our ideas, it is important to be on our guard, lest an incorrect set of terms should produce inaccuracy in our opinions. I am not unaware, that a scientific nomenclature for the appearance of the clouds has been attempted by Mr. Howard, but I hope I shall not be accused of presumption, if I give it as my opinion, that his set of names is much too confined to be of any great use, and that the hypothesis on which he proceeds is not entirely correct. Mr. Howard's.

It will be perceived, that, according to my view of the subject, the science of meteorology must be advanced, not only by accurate observations of individuals, but by the comparison of observations made in different places. It was in a great measure from a conviction of this truth, that I have been induced to lay those remarks before the public, in hopes that some of your readers, in different parts of the kingdom, might cooperate with me in my plans. I should wish that your journal might be made the medium of communication, for there is certainly no other work, that on every account is so well adapted for this purpose. Improvement of meteorology.

I am, Sir,

Your obedient servant,

J. BOSTOCK.

Liverpool,
Mar. 28, 1810.

II.

On some Improvements in the Electrical Machine. Communicated by Mr. J. CUTHBERTSON.

To Mr. NICHOLSON.

SIR,

SOME time ago I was informed by a friend of mine, that Professor Copland of Aberdeen had mentioned to him a valuable improvement, which he had made on my electrical machines. I requested my friend, to procure me the particulars Improvements made in the electrical machine by Prof. Copland.

culars of this improvement; and he soon obtained for me the enclosed paper, which I believe, is in the Professor's own hand writing. I have made some practical remarks on this paper, which may be useful to electricians, and I have also added a hint of an extraordinary improvement I am about to make in the plate electrical machine. Should you approve these observations, you will oblige me by inserting them in your valuable Journal.

I am, with due respect,

Sir, yours &c.

54, *Poland Street, Soho.* J. CUTHBERTSON.

The negative power not equal to the positive

Mr. Cuthbertson's electrical machine is upon the whole the best I ever met with. The only fault I found was, that from insulating the whole machine, so many inlets were given to the fluid, that the negative power of it was by no means equal to the positive. His winch or handle is an insulating one, but it passed so near the mahogany pillar twice in every revolution, that a flash was seen from the handle to the pillar perfectly visible in the dark, and the effect of which I felt nearly to the shoulder. To remedy this I added a glass handle surrounded by a disc about seven inches diameter and of the same piece with the handle.

owing to the hand passing so near the pillar in turning the winch.

A glass handle and disc added to prevent this.

Within the hollow part of the handle was cemented a brass conical socket for receiving the steel pin of the former wooden handle, and a nut to prevent its coming off. The glass disc thus entirely prevented any supply of fluid to the machine by the hand. I now perceived however in the dark a constant stream of the fluid entering by the small brass ball and wire, which supports the silk flaps of the upper rubbers. I therefore took it away, and supplied its place by a solid glass rod.

Farther alteration.

The negative power of the machine was thus more than doubled, or it charged a vial negatively with less than one half of the number of turns it did when I received it. I indeed still perceived some appearances of light, at times, on the edges and angles of the wood work, but by rounding these more, or adding pretty large brass balls contiguous to them;

Edges and angles of the wood work rounded.

them; no light is now visible in the dark, and the negative power seems fully equal to the positive.

The rude sketch, Pl. I, fig. 5, will give some idea of the manner of fixing the handle with its disc. A brass cap is fitted, by grinding, over the inner nut and screw before cementing, to prevent their being fixed by the cement. It also serves as a reservoir. P. COPLAND.

Observations on Mr. Copland's Paper.

The machine described in the Professor's paper was not originally made for him, but for a gentleman who employed it principally for his amusement. I did not therefore attend particularly to its construction with regard to the perfection of either the positive or negative powers, and they might be imperfect. The Professor appears to think, that the greatest imperfection was the insulated winch which imperfection he has overcome by the application of a disc. This disc is precisely similar to an invention of mine made about 30 years ago, when I resided in Holland. I have given a description and plate of it in the second vol. of my *Eigenshappen der Electricitiet*, printed at Amsterdam, 1782; but on account of its awkward appearance, and risk of breaking, I left off using it, and have since employed a simple insulated winch, placed at a greater distance from the mahogany pillar of the machine, and this I find a more perfect remedy.

The improvement had been employed before,

but discused.

With respect to the other improvements the Professor imagined he has made, and proved by experiment; I am inclined to think he has been deceived, because the negative part of an electrical machine, with such imperfect insulations as he describes, will charge a phial to a certain degree, with the same number of turns as one with the most perfect insulation. Such imperfect insulation does not begin to act till the charge attains a certain degree of intensity, till then it keeps pace with the charging power of a machine perfectly insulated. This circumstance does not seem to have occurred to the Professor; had he thought of measuring the advance of the charge at each revolution, before, and after, he had finished his alterations, the result would have been

Farther remarks on imperfect insulation.

more

more satisfactory, and it might then have been known if he had made a real improvement, or not:

Intended improvement of the plate machine.

I shall now proceed to give an account of an improvement intended to be made in the plate electrical machine. I have often thought of attempting to improve the acting power of the plate machines, but in their present state they have given such general satisfaction, that I have deferred this attempt till an opportunity should occur of comparing their acting power with a cylinder properly constructed.

Cylindrical machines generally defective.

All the cylinder machines I had met with were in some way or other defective in their construction, so that I was not satisfied with any of the comparative trials I had hitherto made; for in all these cases the plate machine had evinced very superior action.

One of superior construction.

Some time since however Mr. Singer showed me a cylinder machine of his own construction. In this machine I could find no fault, and its acting power had been considered by most persons as very superior. I was at this time making a two-foot plate machine for Mr. Singer, and we agreed when this should be completed, to compare the acting power of the two machines.

Comparative trial of this with a plate machine

We met accordingly, Mr. Singer managed the cylinder machine, and I excited the plate.

Their powers equal,

The result of a number of experiments proved, that the effect produced by any given number of turns was precisely the same with either machine, so that their acting powers were equal,

but the plate turned with less power.

but the force required to put them in motion was materially different; 8lbs. hung on the winch of the plate, when in a horizontal position, would move it; but it required 14lbs. to move the handle of the cylinder from the same situation. To complete a proper course of experiments, we found it necessary to construct some apparatus, and were therefore obliged to defer the continuation of these inquiries, till we had obtained the particulars alluded to.

The cylinder 14 inches diameter; the plate 24.

It may be necessary to state, that the diameter of Mr. Singer's cylinder is fourteen inches.

The power of [illegible] machine may [illegible] be doubled,

Finding the power of the plate machine equalled by a cylinder, I was incited to attempt an improvement, and a contrivance soon occurred to me, by which I could double the acting power of the plate machine, or make a single plate act equal to a machine with two plates, or equal to two 14-inch cylinders, without being much more laborious

laborious to turn (I communicated this to Mr. Singer, when the same improvement occurred to him.) I am of opinion, that it is possible on this principle, to make one single 2-foot plate machine act equal to four cylinders, or to two double plate machines. If I succeed, I shall be able to equal the power of the large machine, which I made at Haarlem; and that too with one plate only and of much less diameter. If this contrivance should be applied to that machine, the effect must be astonishing; perhaps equal to the production of effects similar to the powers of the voltaic battery. In conjunction with Mr. Singer, I am now engaged in a series of experiments on this subject; should our success be equal to the expectations I have formed, the results will be of the highest interest to the progress of electrical science. When we have completed these experiments, we shall take an early opportunity of communicating them to the public, through the medium of your justly esteemed Journal.

or even quadrupled.

III.

Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By WILLIAM BRANDE, *F. R. S. Communicated by the Society for the Improvement of Animal Chemistry*.*

SECT. I. *Observations on Mucus, and on the Composition liquid Albumen.*

THE results obtained from the chemical analysis of the intervertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the intervertebral substance in fish and quadrupeds†," led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state,

Experiments on mucus undertaken.

* Philosophical Transactions for 1809, p. 373.

† Philos. Trans. 1809; or Journal, vol. XXV, p. 214.

and

and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

Saliva agitated with pure water & filtered.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain*, it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus;

Solution contained saline matter.

but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

Saliva contained 0 02 of phosphate of lime and muriate of soda.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

Mucus of the trachea, and of the oyster.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol, and acids.

Tests of mucus act on the salts.

Finding, therefore, that the reagents employed to detect mucus† act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

Attempt to separate these

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half ounce of

* Nicholson's Journal, Vol. XIV, page 149.

† Nitrate of silver and acetate of lead. Vide Thomson's System of Chemistry, Vol. V, page 500, 3d edition; and Nicholson's Journal XI—251.

water

water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four-inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water*. By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

from the saliva by electricity.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

White substance at the negative side.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

To ascertain this point, an experiment was made on the albumen of an egg.

White of an egg

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

exposed to similar action.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to

The fluidity of albumen perhaps owing to an alkali.

* It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

assume

assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments, which were undertaken to establish so probable an opinion*.

Coagulated albumen boiled.

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, showing that something is retained in solution.

Triturated in water,

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was

yielded alkali.

then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

The solution evaporated.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

Contained albumen.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

Alkaline solution of albumen separates from coagulated white of egg.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids†. This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

White of egg a compound of albumen, al-

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that

* On referring afterwards to Dr. Thomson's System of Chemistry (Vol. V, page 491), I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

† Nicholson's Journal, Vol. XI—246.

when

when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed. kali, and water.

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down. Alcohol coagulates white of egg

1. Five hundred grains of the white of egg were agitated with two ounces of pu e alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen, in passing from the liquid to the solid state, gives it alkali to the alcohol*. by abstracting its alkali.

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali. Acids do the same.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another si- White of egg boiled, cut small, & washed with boiling water, and the filtered fluid electrified.

* When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

milar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

In the negative cup, soda;

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5·5 grains, which had the properties of soda, in a state approaching to purity.

in the positive, muriatic acid.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

Some muriate of soda.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda*. We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present†.

In

* May not a submuriate of soda exist in fluid albumen?

Saline matters in albumen.

† After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration; "as towards the end of the process it appeared vitrified, and glazed with "a melted saline coat, which was, however, easily dissolved by water. "The residuum was again exposed to a long continued red heat, and "again treated with water, till, at length, a few scarcely visible particles "remained, which, as far as such small quantity would permit to be "ascertained, proved to be phosphate of lime. The portion dissolved "by water (which was by much the most considerable) consisted prin- "cipally of carbonate, mixed with a small quantity of phosphate of "soda.

"Five

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterward relate, I find that a battery of twenty-four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

Small electrical power coagulates albumen.

SECT. 2. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

This test applied to other animal fluids.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these reagents producing no effect on the filtered solution just alluded to.

Saliva. All its albumen cannot be coagulated by boiling in water, or by acids.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a considerable

Mucus of oysters contains albumen.

" Five hundred grains of dry albumen afforded 74·50 grains of coal, " of which 11·25 were saline matter."

See " Chemical Experiments on Zoophites, with some Observations " on the Component Parts of Membrane." Phil. Trans. 1800.

considerable and rapid coagulation takes place at the negatively electrified wire.

3. *Mucus of the Trachea, &c.*

Other mucus similar.

The other varieties of mucus, as from the trachea, the nose, &c., agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

Alkalis and acids given out.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

Alkali apparently predominant.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

Is mucus a compound of albumen with soda or its muriate?

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

Bile.

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

Albumen in it variable in quantity,

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0·5 to three per cent, and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

and proportionate to the resin.

The

The electro-chemical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids. Soda, and muriatic and phosphoric acids.

5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cows milk, during one hour, produced the appearance of curds and whey, the principal part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids. Milk.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis. Liquor amnii.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted hidrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause. Its properties.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, A dilute solution of liquid albumen.

learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen*.

7. *Pus.*

Pus.

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

The decomposition of albumen differs according to the power employed.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty-four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole; but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

* The difference in the results of the analysis given in the text, and that of VAUQUELIN and BUNIVA, most probably arises from the liquor of the amnios examined by those chemists not having been perfectly recent, and perhaps mixed with other secretions. See *Annales de Chimie*, XXXIII, p. 270.

IV.

IV.

Comparative Experiments on the Culture and Application of Kohl Rabi, Drum-headed Cabbage, and Swedish Turnips. Communicated by Mr. JOHN SADDINGTON, *of Finchley**.

SIR,

BEING actuated by the most patriotic motives, I beg the favour of you to lay the following communication before the Society of Arts, together with the plants herewith sent. I will endeavour to give you an account, with as much brevity as is in my power to render myself intelligible, of the nature of the soil, the mode of cropping, and the produce thereof. The plot of land being about two acres and a half, and lying on a dead flat, I obtained leave in 1805 to underdrain and break up the same, the grass being sour and useless. Piece of bad grass land underdrained.

I cut two main drains, forty-two inches deep, gradually rising at top to twenty-eight inches, to give a sufficient fall, with sixteen branches twenty-four inches deep, rising to sixteen inches, terminating at top like the letter Y: the drains were wooded with elm, and laid with my own hands; this work was done in February. The soil is a loam, with clay and gravel under. On the 20th of March I sowed three bushels and a half of oats per acre, which produced thirty-nine bushels per acre, weighing forty-one pounds per bushel. The straw was used, as it was threshed, for litter to stalled oxen. The 28th of September seeded with winter tares, four bushels of seed per acre. Ate them off in May with sheep. Two fallow ploughings were given in June and August. About two hundred sheep were brought in at nights by way of fold. The 11th of October sowed three bushels of Thanet wheat per acre. Brined and limed in March, twice fed down with sheep. Produce, twenty-nine bushels per acre, weighing fifty-nine pounds per bushel, and very near three loads and a half of straw per acre. The stubble was mowed and cleared off, and the land got ready for

Soil. Sown with oats, and then winter tares. Fallow ploughed twice. Sheep folded. Sown with wheat.

* Trans. of the Society of Arts, vol. XXVII, p. 75.

Turnips sown. for turnips. Three pounds of seed were sown the last week in August, when the plants were just making their appearance. Two quarters of gypsum were sown by hand to prevent the fly, which had the desired effect.

Gypsum to prevent the fly.

This proved a very valuable crop; having two hundred ewes which gave suck, it was a great acquisition to their milk. This induced me to try three experiments last spring with kohl rabi, or purple turnip cabbage, drum-headed cabbage, and Swedish turnips.

Comparative experiments.

Purple turnip cabbage.

With due respect I beg to recommend to the Society kohl rabi, as a prolific and nutritious plant for the feed of sheep and neat cattle; and green food may be produced by this means from October until May. To ensure a succession of keep, seed should be sown in March, April, and May. The plant bulbs above the ground; the leaf is much like that of beet; it will stand in defiance to the severest frost; and as a proof thereof, I have cut with my knife several of the plants through the crown two inches deep, and they have stood three months afterwards in a sound state; some of them are here produced. The plants may be transplanted like those of cabbage; many of those transplanted at eighteen inches apart, I have found to weigh ten and eleven pounds a piece.—I must now beg leave to introduce my method of cultivation, with the average weight of the crop.

Method of cultivation.

On the 14th of May, I sowed four ounces of seed broadcast, and transplanted about forty-six poles therefrom, on the 18th of June, at twelve inches apart each way. The weight of a square pole is seven hundred and thirty-two pounds, on an acre fifty-eight tuns six hundred eighty-eight pounds, taking each plant to average three pounds. The beauty and regularity of this crop in my idea overbalanced the trouble of transplanting.

Drum headed cabbage.

I likewise sowed upon a bed in the garden, the second week in March, eight ounces of drum-headed cabbage. The fly and slug were very destructive to the plants. I transplanted them the second week in June, upon ridges thirty-six inches apart, the land being dunged at the rate of twelve loads per acre. Some of the cabbages weighed thirty pounds. I think the average about twelve pounds each, or twenty-

twenty-five tuns eighteen hundred sixty-four pounds per acre. The caterpillar was very destructive. I have picked off in a morning as many as would fill a quart pot. Although the kohl rabi was planted near to the cabbage, I never saw a caterpillar upon any of the plants. No caterpillars on the kohl rabi.

In the middle of June, I sewed the remainder of the field with Swedish turnips, but lost two thirds of this crop by the fly. One of the best of the Swedish turnips is here produced, in order to show the great superiority of the kohl rabi, as there is not that waste in being eaten upon the ground, as it bulbs above, and the Swedish turnip in the ground. When the sheep have eaten the turnip level with the ground, and scooped out the inside, the remainder serves as a reservoir for the dirt and filth. The produce of this field has been sufficient for nine score of suckling ewes with rowen for five months. I sent to market, at Christmas last, house lambs fattened with milk only, which weighed eleven stone and one pound each, alive, at eleven weeks old. Should the Society consider these observations worthy of notice, I shall feel myself happy in hearing from you. Swedish turnips.

I am, Sir, very respectfully,

Your obedient servant,

JOHN SADDINGTON.

V.

On the Properties of Furze, or Whins. Communicated by Major SPENCER COCHRANE, *of Muirfield House, near Haddington, North Britain*.*

SIR,

THE Society having honoured me, by publishing in their 25th volume, my communication, stating the advantages arising from the culture of poppies, and that seven ounces of fine salad oil were furnished by expression from two Poppy oil

* Trans. of the Society of Arts, vol. XXVII, p. 77.

pounds of the seed †; I now beg leave to add, that I am informed, considerable quantities of poppy seeds have been lately bought up, in different parts of the country, and the expressed oil from them sold at the price of Florence oil; and that emulsions made from poppy seeds answer in every respect the purposes of those made from almonds.

Furze useful food for cattle.

The following communication may perhaps be deemed worthy the notice of the Society. It relates to the use of whins or furze. Its utility as food for cattle has been long known, though probably not sufficiently appreciated; but as a medicine I never till within a few years heard of it. My information was first received from a gentleman, who has been an officer in the army, a friend and relation of mine; he is seventy-five years of age, and in good health, and what he says may be depended upon. In October 1806, he informed me, that his sight had been much strengthened by drinking an infusion of whin or furze blossoms, dried in the sun in summer. The infusion is made from a tea cup full of the blossoms, in a tea pot, in the manner of tea, and the dose half a tumbler at night. He never had a cough since he first used it, which was fifty years ago; it acts as a diuretic, and by perspiration, and when the dose is increased, promotes sleep. In October 1808, he informed me, that he still continued the use of the whin tea, that he had no cough, and that his skin was remarkably fine and soft, which he attributes to its use.

Infusion of the blossoms strengthens the sight,

and prevents cough.

I have also used the whin blossoms with good effect myself, and can safely recommend them.

Epidemic cold of horses cured by young shoots of furze.

My friend supposes the young shoots of furze may answer, if the blossoms cannot be got. He informs me, that when an epidemical cold came from Germany, and destroyed many horses in England, the east wind continued six weeks, and the infection came over to Ireland, where he had the care of a troop, in so poor a village that he could get neither bran nor malt for mashes, which were ordered for the horses, with sulphur, after bleeding. That he ordered the men to cut furze, and directed them to give it to the horses, after they had beaten it well on the pavement: that at first they had to mix it with oats, but that in two days the horses

† See Journal, vol. XXI, p 157: also vol. XIX, p 282.

devoured

devoured it like clover. That by these means he recovered them all, though every other troop lost two or three; and that his was the only troop in good condition at the review.

I remain with esteem, Sir,

Your sincere and humble servant,

SPENCER COCHRANE.

VI.

Account of several Varieties of British Marble, produced from the Babicomb Quarry, near Teignmouth in Devonshire. By Mr. J. P. Hubbard, Picket Street, Temple Bar.*

SIR,

AGREEABLY to the wishes of the Society, expressed in their list of Premiums, stating that they were desirous to encourage the marble of the quarries of this country, I herewith send fifty various specimens, all arising from one quarry, named the Babicomb quarry, in my possession, situate in the parish of St. Mary Church, near Teignmouth, in the county of Devon, and adjoining the sea. I beg leave to observe, that, though an attempt to introduce this article has once before failed, yet I am confident, if I should be so fortunate as to have my exertions seconded in such a way as the nature of the concern requires, a considerable benefit would ultimately result to the country at large, as well as to myself. The numberless obstacles which I had to encounter, during a period of two years, arising from heavy expenses, and local prejudices, must have damped my exertions, if I had not resolved at the onset to give it a decided trial. Perhaps no period could have offered so eligible as this, for the advantage of the enterprise, owing to the present enormous prices of foreign marble. I am sorry to see, daily, many unaccountable prejudices arise against most articles of the produce of our own country; but I hope

Marble quarry in Devonshire

Foreign marble very dear.

* Trans. of Soc of Arts, vol. XXVII, p 85.

time

time will remove them. It would be presumption in me, to attempt to vie with the finer articles of continental production in this line, but it cannot be denied, that the application of the marbles now produced will be useful, economical, ornamental, and worthy of encouragement. The advantages which would arise to the country at large from a general introduction of this article are very evident, and if I can be favoured with the patronage and support of the Society of Arts &c. towards accomplishing such object, they would meet with the warmest acknowledgments of many individuals beside myself.

Great variety of the British prepared.

I have already prepared a great variety of articles, such as chimney pieces, slabs, &c., of very large dimensions, of these marbles, which are now ready for inspection; and which will show, that I have entered into this business on an extensive scale.

I subscribe myself with great respect,

Sir,

Your obedient servant,

JOHN P. HUBBARD.

Plan of the Society to exhibit British marbles.

The specimens of marble sent by Mr. Hubbard to the Society were each of them eight inches high, six inches broad, and one inch thick, and polished on one face; such are the dimensions pointed out by the advertisement of the Society, in order that a regular range of British marbles may be fixed round the Society's Great Room, to show to the public what our quarries can produce. Mr. Hubbard's marbles were, on being received, referred to the consideration of their Committee of Chemistry, and the following additional information obtained respecting the quarry and produce thereof, viz.—

Extent of the Devonshire quarry.

That the quarry which produced the different specimens is twelve acres in extent.

Varieties.

That marble similar to each specimen can be distinctly procured.

Large blocks,

That Mr. Hubbard had then in his possession columns of red marble, eight feet long, and two feet diameter, and believed that they might be got ten feet long, and five feet diameter,

diameter, and that blocks of other kinds might be got of large sizes.

That he had at that time slabs six feet six inches long, by three feet six inches in width. and slabs.

That the quarry is close to the sea, and a part thereof covered by it at high water, and that he can load vessels direct from the quarry, having made a wharf for that purpose. Convenience of water carriage.

That the quarry is situate about four miles from Teignmouth, and was first opened about sixteen years ago, and was afterward neglected; but that it has been now worked by him for two years.

That the marble is harder in quality as the mine goes deeper, and that some part of it rises fifty feet from the sea.

That the sale price is about half that of foreign marble of similar appearance; that the general price is now about four shillings per superficial foot, and will probably be so reduced as to be delivered at three shillings in London. Price of the marble.

That it will take a finer polish than any other marble found in the kingdom.

That he supposes from sixty to one hundred workmen may be employed in the quarry next autumn.

That chimney pieces made from this marble are not injured from the heat of fire applied near to them, nor liable to crack from alternate sudden changes of heat and cold. Not injured by heat.

That great part of the refuse stones of the quarry will burn to lime, and that such lime is of superior quality to any other on that coast. Lime from the refuse.

The Society having taken into consideration the circumstance of Mr. Hubbard's having carried their views to so great an extent, and of his undertaking being likely to prove highly advantageous to this country, voted to him their Gold Medal, although no specific premium had been ever offered by them for coloured British Marbles. Gold medal voted.

Mr. Hubbard afterwards presented the Society with ten more specimens from his quarry, which with two specimens of Devonshire marble presented by Lord Clifford, and two others presented by Mr. W. Coles, have been framed along the 64 specimens.

the surbase of the Society's Great Room; where it is also intended to place such other marbles, the produce of the British Empire, as may be presented to them, with references to each sample, that the public may know whence each kind can be procured.

VII.

Inquiries concerning the Heat produced by Friction: by Dr. Haldat, Secretary to the Academy of Nancy.*

Heat an object of difficult research,

So much has been done concerning heat in our days by eminent natural philosophers, that the subject would be exhausted, if it accommodated itself so easily to experimental research as many others; and if the fluid, which is pretty generally admitted as the cause of calorific phenomena, could be treated like those elastic fluids, the knowledge of which is at present so far advanced: but, incoercible in the highest degree, and incapable of having either its bulk measured or its weight ascertained, it eludes our research, and this real Proteus escapes into the depths of nature, the moment we attempt to lay hold of it. These properties however, which seem calculated to render it the despair of philosophers, have excited their emulation: but, as they exhibit themselves in different points of view, each has adopted for their explanation that hypothesis, which appears to him the most natural, and their opinions are divided.

and opinions on it divided.

According to the ancients an element.

The ancients explained the calorific effects, with which they were acquainted, by means of a fluid of extreme subtilty and incomparable activity, which gave it the power of attacking bodies, and resolving them into their first principles. They ranked this substance among the elements, of which they composed the universe. This opinion, variously modified at different times, was generally adopted till the age of Descartes; when that great genius, sent to renovate the sphere of science, represented these phenomena as a simple modification of matter, which all bodies were susceptible

This controverted by Descartes.

* Journal de Physique, vol. LXV, p. 213.

tible of, and which consists principally in the extreme attenuation of their constituent molecules. Philosophers then were divided into two classes, they who made the calorific phenomena to depend on the action of a substance of a peculiar nature, and which has been called matter of fire, or caloric; and those who supposed them to depend on a certain mode of being of the particles of bodies. The former opinion, which was most generally adopted, had become almost universal, since the phenomena of heat, having been more thoroughly studied, had been presented as dependant on a substance that might be transferred from one substance to another, in the same way as a fluid is poured from vessel to vessel; that might be combined with substances, and the combination of which produces remarkable changes; and lastly that might be set free from its combinations, and determined to others by the same means as are employed to produce similar changes in all substances. There seemed to remain no doubt of the substantiality of caloric, and philosophers seemed employed only in making better known a substance, the existence of which appeared to them sufficiently demonstrated, when count Rumford, dear to his country by his beautiful discoveries, and not less dear to humanity by his philanthropic labours, began to excite fresh doubts of the existence of caloric, and again place the phenomena ascribed to it among the modifications of which substances are susceptible.

The ancient opinion prevalent,

till opposed by count Rumford.

The property of friction to develope heat had long been known; but this fact, so deserving of attention, had not yet been subjected to proper examination. Count Rumford, having made a blunt borer turn in a brass cylinder immersed in water, obtained from it a quantity of heat so disproportionate to any thing the brass could have lost, that he thought himself warranted to infer, that this heat could not have arisen from any condensation of the metal, but must have been produced by the agitation of the particles communicated to the water in the manner of sound. This conclusion however, which tends entirely to overthrow the theory of caloric, has not appeared to be legitimately deduced from the facts; and Mr. Berthollet has refuted it in a note

Heat produced by friction.

to

to his Chemical Statistics, vol. I, p. 247 *. Thus the opinion of philosophers remains wavering between two theories diametrically opposite, and each supported by respectable authority. Accordingly I have undertaken the following experiments, with the view to add some facts to those, that will serve perhaps some day to elucidate more completely this important question. The experiments of count Rumford, executed on a large scale, were well calculated to give rise to conspicuous phenomena; but they appear to me, not to have been varied sufficiently. This is the object I proposed to myself, not merely the rubbing of bodies of different kinds, but likewise by varying all the circumstances that could concur in the developement of heat.

Acquires further experiments to detect its source.

Apparatus for this purpose.

The apparatus I employed consists of a small cubical oaken box, very firmly put together and cemented, in which an axis turns vertically. The lower end of this axis rests in a copper socket fixed to the bottom of the box, and on the opposite end a grooved wheel is securely fastened. In the upper third is a rim, resting on a bed of copper fixed to the cover of the box: and to the lower third is fixed a piece of copper furnished with rims, to retain cylindrical pieces of metal fitted to it. These pieces are 6 cent. 5 mill. [$2\frac{1}{2}$ inches] in diameter. On the convex surface of these hollow cylinders, the friction is produced by means of a spring fastened horizontally in the inside of the box. This spring receives at one extremity rubbers of metal, which are fitted to it by means of a groove; while a screw, passing through the box, gives the spring the tension necessary to press the rubber against the surface of the cylinder. A graduated arch fitted to the spring indicates in weights the force produced by its tension. The rotatory motion, that produces the continual friction, is given by an endless cord passing round the wheel on the axis and the large pulley of an iron-turner's wheel. The diameters of these are to each other as one to four, so that the velocity of the smaller is four times that of the larger, and by turning this only once round in a second, the smaller will make four revolutions in this period; so that the

* So it is in Rees's New Cyclopædia, art. Caloric. Trans.

axis,

axis, moving with the same quickness, produces a friction, the velocity of which is more than 84 cent. [32·8 inches] in the same space of time. This velocity, sufficient to produce sensible effects, is that which I have generally employed in my experiments. The other parts of the apparatus are hollow cylinders, equal in surface but of different materials, and rubbers of copper and of steel 3 cent. [1·2 inch high.]

The heat developed by the friction of the pieces of this apparatus is employed to raise the temperature of a mass of water of 3 dec. 664 cent. cubic measure [216 cub. inches], which the box is capable of containing, and this temperature is measured by the thermometer immersed in it. The water employed was in general nearly of the temperature of the air of the room, in order to avoid the influence this might have had on that of the water during the course of the experiment. This influence was farther diminished by preventing the air from being renewed, and shortening the period of the operations.

The heat produced raises the temperature of water in the apparatus.

Exp. 1. The first trial was made with a cylinder and rubber of brass. These pieces, before they were subjected to friction, were accurately weighed both in air and in water. Their temperature was 4° [39·2 F.]: the spring acted with a force equivalent to a pressure of 20 kil. [44lbs.]: and the mean velocity was 60 turns of the greater wheel in a minute. After 15 minutes continued friction, the temperature of the water was 6° [42·8]; and it was pretty regularly raised 2° [3·6°] in every interval of 15 minutes, so that at the expiration of 70 min. the temperature was 13° [55·4°]. The cylinder and rubber, weighed anew in air and water, showed no diminution in bulk or weight, that the balance could detect, though it was sensible to half a grain. The two pieces rubbed together however, exhibited a polish at the points in contact, which indicated a slight loss of substance. This was chiefly visible on the rubber. Thus a surface of 3 cent. [1·2 inch] broad, and 21 cent. [8·3 inches] long, rubbed with a velocity of 84 cent. [32·8 inches] per second, produced a quantity of heat capable of raising the temperature of 3 dec. 666 cent. cub. [216 cub. inches] of water 9° [16·2°]; or in other words of melting more than an eighth part of ice, though the quantity of metal detached from

Exp 1 with a brass cylinder and rubber

from the surface was less than half a grain, and the condensation inappreciable.

Exp. 2. Cylinder of lead.

Exp. 2. For the brass cylinder of the preceding experiment I substituted one of lead. The two pieces were previously weighed both in air and water as before. The temperature of the air was 9° [48·2°], that of the water employed 7° [44·6°]. The thermometer, observed every 15 minutes, exhibited the following progression in the temperature of the water 10°, 12°, 14°, 16° [50°, 53·6°, 57·2°, 60·8°]. The experiment continued 75′. Neither the leaden cylinder nor the rubber showed any perceptible diminution of weight or bulk. The point of contact was distinguishable only by a very slight mark on the rubber, and the polish of the surface of the cylinder: whence it appears, that, without any perceptible alteration in its bulk or weight, a cylinder of lead, the density of which is to that of copper as 11·352 to 8·788, produced an equal quantity of heat.

Produced an equal quantity of heat.

Exp. 3 Cylinder of tin.

Exp. 3. This result, the reverse of what the theory of friction seemed to indicate, induced me to make still farther inquiry into the influence of the density of the body rubbed by employing a metal of less density than that of copper; and accordingly I substituted for the preceding a cylinder of tin, the density of which is to that of lead as 7·291 to 11·352. The circumstances being the same, the temperature of the air and water 11° [51·8°], the acquired temperature gave the following progression: 13°, 15°, 17°, 18° [55·4°, 59°, 62·6°, 64·4°]. Hence it follows, that in the same time a cylinder of tin gives but $\frac{7}{9}$ of the heat produced by one of copper, while the latter gives an equal quantity to that produced by lead, though its specific gravity is only about $\frac{8}{11}$ of this metal. The volume and weight were found not to be sensibly changed.

Produced only 7 9ths as much heat.

Exp. 4 Cylinder of zinc.

Exp. 4. A metal, the density of which is still less than that of tin, but the hardness of which is much greater, and which possesses little malleability, zinc, was substituted for this metal. Rubbed during the same time, with the same velocity, and under the same pressure, the temperature of the air and that of the water employed being 10° [50°], it exhibited the following progression: 12°, 14°, 16°, 18°, 20° [53·6°, 57·2°, 60·8°, 64·4°, 68°]. The zinc cylinder then, less dense than those of copper and lead, afforded a much larger quantity of heat.

Produced more heat.

Exp.

Exp. 6 and 7. I sought to confirm the influence of pressure on the evolution of heat by two experiments, in one of which I made it four times as great as in the other. For this purpose I employed the cylinder of brass, and the rubber of the same metal; and the velocity being equal, the pressure was at first 10 kil. [22 lbs.]. The temperature of the water rose only 1° [1·8°] in 30 minutes. Having afterward rendered the pressure equal to 40 kil. [88 lbs.], the temperature acquired by the water in the same space of time was six times greater, or 7° [12·6°].

Exp 6 and 7. Brass.

Quadruple pressure produced septuple heat.

Exp. 7. The cause, to which we ascribe the most general influence on the developement of the heat produced by friction, is the erosion of the surface rubbed, the particles of which are separated with violence. In order to ascertain this influence, I employed a rubber of steel, cut so as to resemble a bastard file; and applied it to the surface of the cylinder with a force equal to 20 kil. [44 lbs.]. Having accurately weighed the cylinder in the air, I turned it with the same velocity as in the preceding experiments; and in the space of 60 minutes the temperature of the water was raised only from 14° [57·2°] to 18° [64·4°]. The cylinder had lost 3 decagr. [463 grs.] of its weight. This quantity of metal, a thousand times as much as that lost by the friction of the same cylinder against a rubber of smooth polished brass, in Exp. 1, gave but half the quantity of heat in the same time, though taken off by an equal pressure.

Exp 7. Brass rubbing against a steel file

produced but half as much heat as when rubbing against smooth brass.

Exp. 8. In the 8th experiment I proposed to ascertain the influence, that the free communication of the parts of the apparatus with the surrounding bodies by means of good conductors of heat, or their insulation by bad conductors, might have on the production of heat. With this view I placed the apparatus in a deal box, in which it was kept at the distance of a decim. [3·9 inches] from every side by pieces of wood half charred. The interval between the two boxes was filled with smallcoal, forming a stratum on all sides near four inches thick. I employed the brass cylinder and rubber, subjected to the same pressure, and rubbed with the same velocity. The time of the experiment was divided into three equal portions of 30 min. each. In each of these periods the temperature acquired by the water in

Exp. 8 Brass, surrounded by a bad conductor of heat,

the inner box was pretty regularly 3° [5·4°]. Though the communication with surrounding bodies was alternately interrupted and restored, it is to be observed, on comparing this experiment with the first, that, if the quantity of heat were equal, it was in a third longer time; whence it follows, that in the same time it was a third less.

heated more slowly.

Experiment 9. Brass insulated

Exp. 9. The electric fluid, as well as caloric, is developed by friction, and generally propagated with more facility; and as these two fluids have some analogy to each other, the former may be suspected, to have some influence on the developement of the heat, and to furnish the matter. To appreciate this hypothesis, I repeated the preceding experiment, insulating the apparatus by nonconductors, and alternately establishing its communication with the ground. For this purpose it was placed in a box of very dry deal coated with resin, from every side of which it was kept at the distance of a decimetre [3·9 inches] by pieces of wood baked in an oven, and immersed while hot into boiling gum lac. The whole was placed on an insulating stool with glass feet. The experiment, which continued an hour, was divided into four equal portions of time, in which, though the communication with the earth was alternately interrupted and restored, the quantities of heat appeared equal. In the 60 min. the water acquired 6° [10·8°], whence it would appear, that insulation, either by nonconductors of electricity, or by bad conductors of heat, diminishes the quantity of the heat produced by friction.

Electricity appears to have no effect in the production of heat by friction.

Exp. 10. An iron wire compressed by repeated blows.

Exp. 10. Of all the causes suspected to influence the production of heat in our experiments, none appears more powerful, than the condensation of the particles of bodies arising from the pressure necessary for the friction. It is to this cause too, that Mr. Berthollet has thought fit to ascribe it; but as its influence rests solely on theory, I attempted to confirm it by experiment. For this purpose I constructed a small oak box, capable of containing a cubic decim. [61 cub. inches] of water. The four sides were firmly united by iron screws. The bottom was closed by a block of oak 3 dec. [11·8 inches] long, one end of which was rabetted to receive the sides, and these were fastened to it likewise by screws. The whole was covered with a cement impenetrable

to-

to water. On the bottom, namely in the end of the block, and perpendicular to its fibres, was fixed a small steel anvil of 3 cent. 25 mil. On two opposite sides, level with the anvil, were two leather boxes, by means of which a wire could traverse the box, without letting out the water. This wire, part of which rested on the anvil, was subjected to strong compression by means of a prismatic stamper of steel, which descended through the cover of the little box, and moved in a groove, that directed its lower extremity to the anvil. This stamper, driven by hard strokes with a hammer, transmitted to the wire, on which it rested, the blows it received, and compressed it. The hammer I employed weighed 2 kil. [4 lb. 6½ oz.] At every stroke the wire was advanced two thirds of the breadth of the anvil. The metal subjected to experiment was an iron wire one third of a line in diameter, and of the weight of 366 grains [300 grs Eng.]. Compressed throughout by the mechanism I have described, it formed a band a little more than a line broad, and its length was increased; but its specific gravity had acquired no perceptible increase. The temperature of the water employed to collect the caloric evolved by this condensation rose only 2° [3·6°]. As my balance detects a variation of less than half a grain, a change in the density, less than would produce a difference of half a grain in the specific gravity of 366 grains of iron wire weighed in water, extricated a quantity of heat that raised the temperature of a cubic decimeter of water one degree, and consequently would have melted more than an eighteenth part of ice.

If the experiments I have described had not all the success I expected to determine the cause, that produces the heat extricated in the friction of substances, they appear to me not destitute of utility. They not only confirm the experiments of count Rumford concerning the astonishing quantity of heat produced by friction, but they prove (allowing for the irregularities unavoidable in experiments of this sort), that this quantity of heat is modified by the nature of the substances rubbed, that it is not in the ratio of the surfaces, for equal surfaces have given unequal quantities; and that it is not in the ratio of the number of particles rubbed off, or of the density; for lead, the density of which

Friction produces an astonishing quantity of heat,

and this is neither in the ratio of the surface rubbed, the density of the substance, nor the quantity abraded

which is greater than that of copper, gave but an equal quantity of heat, while zinc, which is less dense than either, gave a greater quantity.

Influence of pressure,

The influence of pressure is shown by the 5th and 6th experiments, and as its effect on compressible bodies is necessarily to crush and condense them, does it not seem to indicate the approach of the particles as the cause of the extrication of heat? Yet as this condensation must be the greater, in proportion as the same pressure is exerted on a small number of points at a time, and is employed to detach particles, that cannot be separated till they have experienced a considerable approximation of their parts, the 7th experiment, in which the steel file-cut rubber detached a considerable quantity of particles of copper, should have produced a proportionate quantity of heat: yet it gave less by half than the friction with a smooth rubber.

and of condensation.

The influence of condensation on the production of heat is rendered still more uncertain by the 10th experiment, in which the iron wire, compressed and flattened by strokes of a hammer, ought to have set free a quantity of heat so much greater, in proportion as the force employed to effect the compression was more powerful. In vain would it be objected, that there was no condensation, the specific gravity of the wire not appearing to be changed: the electricity and fragility it had acquired, certain signs of compression, leave no doubt of the reality of this condensation, though it was too little to affect the hydrostatic balance. The condensation was greater than that produced by simple pressure, yet the quantity of heat extricated was less. These facts, the reverse of what might have been expected, render the explanation of the calorific phenomena produced by friction very difficult. They would seem to favour the opinion of count Rumford; but the materiality of heat is confirmed by so many arguments, that we ought not to relinquish a theory so fertile in useful explanations, before fresh experiments have completely elucidated this important point.

Difficulties in the hypothesis of material caloric,

In admitting, that the calorific phenomena, produced by friction, depend on the extrication of caloric, expelled from the pores of substances by forcing the particles nearer together, how is it that the particles, in resuming their former situation,

tuation, which necessarily takes place in elastic metals like zinc, do not resume the quantity of heat they have imparted to the water? and if this hypothesis explain the little heat of the 10th experiment, how does it apply to the great quantity produced by the simple pressure in the first, second, and third experiments? In discussing these facts, we are led to the following consequences. If the calorific phenomena produced by friction depend on a particular fluid called into action by this process, either this fluid is extricated from the pores of the metal by condensation, or it is drawn off and taken away from the surrounding bodies like the electric fluid. In the first case, the heat must diminish by condensation, must follow the inverse ratio of the density, and must be exhausted; in the second it must be modified by the insulation of the substances rubbed: which did not take place either in my experiments, or in those of count Rumford. If on the contrary these phenomena are produced solely by the internal agitation of the particles, the quantity of heat should diminish by condensation, and exhibit some proportion to the density and more particularly to the elasticity of the metal. Such are the doubts, that still obscure the question concerning the cause of the heat produced by friction, and require farther experiments. It is sufficient for me to have opened the way, and shown its importance and its difficulties.

and in that of vibrations of the particles.

VIII.

Abstract of a Paper read to the Institute the 21st of July, 1807, by Messrs. FOURCROY *and* VAUQUELIN, *on some Bones found in a Tomb in the Church of St. Geneviva**.

THESE bones were in general extremely fragile; but their heads were particularly so, owing to their great porosity.

Bones very tender,

Their colour is purple, much resembling that of dried

purple,

* Annales de Chimie, Vol. LXIV, p. 190.

wine

wine lees. This colour is much stronger in the body of the bones, than in their head, where on the contrary it is brownish.

filled with crystals,

In the body of the bones, as well as in their heads, are a number of white shining crystals, which have the appearance of sulphate of lime. These crystals, by forming in the interior of the bones, have raised their laminæ, and rendered them so brittle.

700 years old.

They are supposed to have been buried in the eleventh century, consequently to be at least 700 years old.

Boiled in water, rendered it acid,

Exp. 1. Being reduced to powder, and boiled in 300 parts of distilled water, they imparted to it a very pleasing red colour. The decoction was slightly acid. On the addition of ammonia its red colour was immediately destroyed, and a greenish precipitate formed, which became bluish in drying. The base of this precipitate was ammoniaco-magnesian phosphate.

lost 0·35, and much colour.

The substance of the bones thus boiled had lost only 35 hundredths of its weight. Its purple colour was exceedingly diminished.

Residuum dissolved in nitric acid.

The portion not dissolved by the water was in great measure dissolved in weak nitric acid witout any effervescence. Nothing remained but a few hundredths of a white powder, which was mingled with some brown membranes. This residuum will be noticed hereafter.

Phosphate of lime.

What the nitric acid had dissolved was phosphate of lime, mixed with a small quantity of red colouring matter.

The bone treated with nitric acid.

Exp. 2. A piece of one of these bones immersed in weak nitric acid was soon dissolved, leaving only a soft red substance, which retained nearly the bulk and shape of the fragment. The nitric acid itself was turned red. The substance mentioned dissolved in alcohol, and imparted to it a very fine red colour, perfectly resembling that of archil dissolved in the same menstruum. When the substance in question was dissolved in alcohol, some brown flocks were left, being the remains of the membrane of the bone, that had escaped decomposition.

Residuum dissolved in alcohol gave out a red colour like archil.

Colouring matter.

The matter that gives the purple colour to the bones therefore is soluble in alcohol, and even in water. Alkalis mixed with it give it a very fine green colour, perfectly similar

milar to the tint observed in some decayed timber. This last mentioned substance, being dissolved in alcohol, likewise communicates to it a deep purple colour; but there is this differerence, it becomes green again on the addition of an acid, while that of bones, which is turned green by alkalis, has its red colour restored by acids.

The matter remaining after the action of alcohol on this red colour, that is to say the membranes, being subjected to the action of heat in a platina crucible, emitted a fetid vapour, then burned, and left only a few grains of sand. Membrane.

Without pretending absolutely to determine the origin of this red matter, Messrs. Fourcroy and Vauquelin are of opinion, that it proceeds from the decomposition of the animal substance. In fact we perceive many organized substances produce by their spontaneous decomposition, or putrefaction, colours that did not exist in them before. They themselves have described a colour, developed by the putrid decomposition of the gluten of wheat, which appears very similar to that of these ancient bones. Colouring matter supposed to arise from decomposition of the animal substance.

Exp. 3. As to the white shining crystals mentioned above, and found both on the surface of the bones and between their laminæ, Messrs. Fourcroy and Vauquelin satisfied themselves, that they were formed of lime, phosphoric acid, and a little magnesia; and consequently that they were phosphates of lime and magnesia. The lamellar structure of these crystals, their brilliancy, and their flexibility between the teeth, had led those chemists at first to suspect, that they were sulphate of lime; though they were puzzled to explain the origin of the sulphuric acid. But having separated with great care a gramme of these crystals, they put them into diluted nitric acid, which dissolved them speedily and easily: and this solution was not precipitated by nitrate of barytes, which must inevitably have happened, had the crystals been sulphate of lime; but a copious precipitate was thrown down by oxalate of ammonia, and by ammonia alone. This salt fuses before the blowpipe much more easily than sulphate of lime; it diffuses, when kept long in fusion, a phosphoric light; and forms a semitransparent glass, which sulphate of lime does not. The crystals were phosphate of lime and of magnesia.

Thoroughly convinced by these experiments, that the matter To account for

their solubility in water, ter in question was composed of phosphoric acid, lime, and a little magnesia, it remained for Messrs. Fourcroy and Vauquelin to find how this compound could dissolve so copiously in water. they were boiled, With this view they boiled a gramme a long while, and several times in succession, in 400 parts of water. The first boiling had a rosy hue, evidently reddened paper tinged with litmus, and was copiously precipitated by alkalis, lime, precipitated by alkalis, and other alkaline substances. These precipitates had all the properties of neutral phosphate of lime.

and the fluid evaporated, After having precipitated by ammonia a pretty large quantity of the solution of this substance in water, they evaporated the fluid to dryness. During the evaporation some which left phosphoric acid. ammonia was given out. The matter that remained had a brown colour, attracted the moisture of the air, was acid, and formed a copious precipitate with limewater; which proved, that it was phosphoric acid.

Consequently superphosphate of lime with a little magnesia. It is evident from the experiments related, as well as from several others, that the white crystalline substance, which covers and penetrates the bones found in the church of St. Geneviva throughout their whole extent, is a true acidulous phosphate of lime, containing a small quantity of phosphate of magnesia.

Whence comes the surplus of phosphoric acid? But how shall we account for the formation of the phosphoric acid? We know, that this acid does not predominate in bones either dry or recent; and that, on the contrary, it is always accompanied witha a certain quantity of carbonate of lime. From a new acid forming, and taking away part of the lime? Messrs. Fourcroy and Vauquelin see no way of explaining this singular phenomenon, but by admitting, either that an acid is formed by the decomposition of the animal matter, which acid not only saturates the carbonate of lime, but takes away a portion of lime from the phosphoric; or from the decomposition of phosphorus contained in the animal matter? or that phosphorus existed in the animal matter, which has been converted into phosphoric acid, and afterward combined with the phosphate of lime, forming a superphosphate.

The latter more probable. The latter appears to these gentlemen more natural, since it is more agreeable to the laws of affinity, and particularly to the discovery of phosphorus in animal substances. In fact no acid could have been formed by the decomposition of the animal matter but vinegar, or some other weak

weak animal acid: but these acids could not take lime from the phosphoric; and besides, as we find nothing of this acid, or of the lime wanting to the bones, we must suppose, that they had been volatilized together, which is improbable.

If this supposition be as true, as it appears likely, a pretty large quantity of phosphoric acid must have been formed, since it was sufficient to saturate the carbonate of lime, which no doubt existed in the bones of the ancients, as it does in those of the moderns, and to convert part of the phosphate of lime into an acidule. The existence of the acidulous phosphate of lime in nature is not a new fact, for Messrs. Fourcroy and Vauquelin have sometimes met with intestinal calculi of herbivorous animals in this state, and exhibiting a decided crystallization: but they say, they never saw any, in which the acidity was so striking, or which were consequently so soluble in water.

In this case a large quantity of phosphoric acid formed.

Superphosphate of lime in intestinal calculi.

The examination of these bones informs us of several things, which appear to merit some attention. The first is the formation of a certain quantity of phosphoric acid by the decomposition of the animal matter, in which the phosphorus was indubitably* contained; the second, that this animal matter, by some change not perfectly known, gave rise to a very fine red colour, which is turned green by alkalis; the third, that this colouring matter had been preserved for so many ages†, without being destroyed, which appears to have been owing to its combination with the phosphate of lime.

Remarks.

* Just now the existence of the phosphorus in the animal matter was only presumed, as the most probable way of accounting for the existence of the superphosphate of lime.

C.

† This is a gratuitous supposition. If it did not exist originally in the bones, which is not supposed, who can say, how rapid or how slow the process was by which it was formed? and if a series of decompositions took place, it must evidently have been the result of the last of these.

C.

IX.

IX.

Experiments on the Tartarous Acid, and particularly on the Acid it affords by Distillation in the dry Way: by Messrs. FOURCROY *and* VAUQUELIN*.

Pyroligneous & pyromucous acid found to be compounds of the acetic, and the pyrotartarous supposed to be the same,

MESSRS. Fourcroy and Vauquelin having examined the pyroligneous and pyromucous acids in the year 8, found that they were formed of acetic acid and an empyreumatic oil. Having afterward examined the pyrotartarous acid, they perceived, that it was volatile; and that with potash it composed a salt in foliated crystals with the appearance of mother of pearl, attracting moisture from the air, having a pungent and acid taste, totally soluble in alcohol, and emitting a pungent acid smell when acted on by sulphuric acid. From these experiments they inferred, that tartar afforded by distillation the same acid as gum, starch, wood, &c.. and in fact it was scarcely possible, to form any other conclusion; for beside the properties of the pyrotartrite of potash, which are nearly the same as those of the acetate, their opinion was supported by analogy.

but this questioned.

But Mr. Gehlen having said in a letter printed in the Annales de Chimie for October 1806, that he could not believe the pyrotartarous acid to be the acetic, because, after slow evaporation it left a crystallized residuum, which differed too from tartarous acid, Messrs. Fourcroy and Vauquelin have examined the pyrotartarous acid anew, and the following is the result of their inquiries.

Pyrotartarous acid combined with potash.

1. The acid liquor obtained by the distillation of tartar being saturated with carbonate of potash, a part of the oil dissolved by this acid was precipitated in the form of a brown resin, yet a large quantity remained in the new compound.

The salt crystallized

2. This compound, evaporated to dryness and redissolved several times in water, yielded a salt of a brownish colour,

* Annales de Chimie, vol LXIV, p. 42.

a hot

a hot and pungent taste, and a foliated form, like acetate of potash.

3. This salt precipitated the nitrates of mercury and of silver in white scales; but it precipitated the solution of acetate of lead also, which acetate of potash does not. Precipitates acetate of lead.

4. Exposed to the fire it swelled up, and was carbonized.

5. Distilled with diluted sulphuric acid by a gentle heat, it turned black, and toward the end of the process yielded a white sublimate, which adhered to the whole surface of the retort in the form of scales. The liquid, that came over before the sublimate appeared, had a very decided acidity, which was not owing to the sulphuric acid employed; but it had only a very slight smell of vinegar. Distilled with sulphuric acid.

6. This process, the distillation of the salt formed by the pyrotartarous acid and potash, afforded a singular appearance. The acid liquid just mentioned contained at the bottom a large globule of another liquid, with a slight yellow tinge, that rolled about when the vessel was moved without mixing with the liquid containing it. It resembled phosphorus melted at the bottom of water. As it was night, Messrs. Fourcroy and Vauquelin stopped the vessel very close, in order to examine it more accurately the next day: but in twelve hours it was not to be seen, the heavy globule having mixed with the other liquid during the night. Singular phenomenon.

7. The retort being cut, the crystals were separated as completely as possible, and exhibited the following properties. Crystals produced.

a. Their taste was extremely acid. Their properties.

b. They readily melted, and evaporated in white fumes, when placed on a heated substance.

c. They dissolved copiously in water, and crystallized again by spontaneous evaporation.

d. Their solution did not precipitate that of acetate of lead, or that of nitrate of silver, but it precipitated nitrate of mercury. However, some time after this acid had been mingled with a solution of acetate of lead, needly crystals were found it arranged in plumes.

e. The solution of this acid partly saturated with potash does not furnish an acidule as the tartarous acid does, but

it immediately precipitates acetate of lead, though the sublimed concrete acid does not, when employed singly and pure.

f. The neutral compound of this acid with potash, in a state of deliquescence, is soluble in alcohol. It does not precipitate the salts of barytes or of lime, as the alkaline tartrites do.

g. The liquid obtained in the same process as the crystals just mentioned, being evaporated with a very gentle heat, likewise furnishes crystals, which have properties precisely similar to the former.

The pyrotartarous a distinct acid.

From these experiments it follows, that the pyrotartarous acid is a peculiar acid: that it differs from the acetous in being less volatile, in having less smell, in crystallizing by evaporation, and in its combination with potash precipitating acetate of lead: that it differs from tartarous acid in not precipitating lime, barytes, or acetate of lead: and that it does not form with potash an acidule of difficult solubility.

Distinguished from the acetic.

Messrs. Fourcroy and Vauquelin made one experiment, which incontestably shows, that the pyrotartarous acid is not acetic acid changed in its properties by combining with the oil produced at the same time. They repeatedly distilled concentrated acetic acid with the oil procured by the dry distillation of tartar. They then combined this acid with potash, distilled the salt thus produced with sulphuric acid, and obtained nothing but empyreumatic vinegar.

Some acetous acid formed.

It appears however, that a small quantity of acetous acid is formed in the distillation of tartar, from the pungent smell emitted when sulphuric acid is poured on the salt formed by combining the empyreumatic acid with potash.

Pyromucous & pyroligneous compounds of the acetic.

The pyromucous and pyroligneous acids, being subjected to fresh experiments, comported themselves like acetic acid holding in solution empyreumatic oil.

So is the formic.

The authors have examined anew the acid of ants, which Mr. Gehlen says is different from vinegar; and they have confirmed the identity of these two compounds, at the same time showing, that the former contains a little phosphoric acid combined with an animal matter.

Analysis of cream of tartar.

They conclude their paper with an analysis of tartar, or the supertartrite of potash. They found, that 1000 parts of this

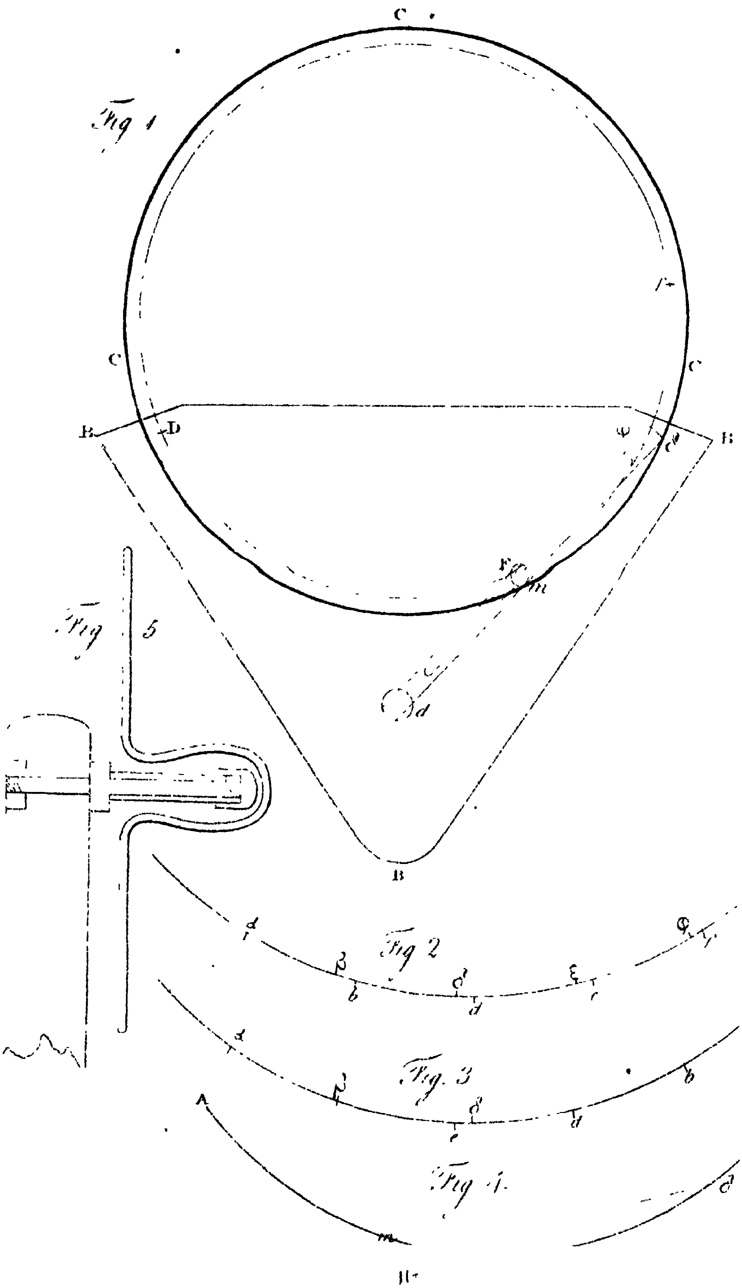
Fig 1
C
C
C
B
D
B
F
m
d
B
Fig 5
Fig 2
β
b
d
ξ
c
Fig. 3
β
e
d
b
A
Fig 4.

this salt, of the finest quality, yielded by distillation, exclusive of the acid product and coal,

Pure dry carbonate of potash, ...	350 parts
Tartrite of lime	6
Silex...........................	1·2
Alumine	0·25
Iron mingled with manganese	0·75

X.

On an Improvement in the Manner of dividing Astronomical Instruments. By HENRY CAVENDISH, *Esq. F. R. S.**

THE great inconvenience and difficulty in the common method of dividing arise from the danger of bruising the divisions by putting the point of the compass into them, and from the difficulty of placing that point midway, between two scratches very near together, without its slipping towards one of them; and it is this imperfection in the common process, which appears to have deterred Mr. Troughton from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume †. This induced me to consider, whether the abovementioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently, that the great objection to the old method of dividing is entirely removed.

Inconvenience of the common method of dividing instruments.

Method of remedying this.

In this method it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let C C C (Plate I, Fig. 1,) be the circle to be divided, B B B a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: $d\delta$ is the beam com-

Apparatus for dividing astronomical instruments described.

* Philos. Trans. for 1809, p: 221.

† See Journal, vol. XXV, p. 1, and 100.

pass,

pass, having a point near δ, and a microscope *m* made to slide from one end to the other. This beam compass is supported at *d*, in such manner as to turn round on this point as a centre, without shake or tottering; and at the end δ it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of *d* from the centre of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which *d* is supported had best be made to slide nearer to, or farther from, the centre; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of *d* from the centre of this circle shall not alter by sliding the frame.

Method of using it for division by continued bisection.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and *f* be two points on this limb, which are to be bisected in φ. Take the distance of the microscope from the point nearly equal to the chord of *f* φ, and place *d* so that the point and the axis of the microscope shall both be in the circle, in which the divisions are to be cut. Then slide the frame B B B, till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the centre *d* till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point *f*, make another faint scratch with the point; which, if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point midway between them will be the accurate bisection of the arch F*f*; but it is unnecessary, and better not to attempt, to place a point between these two scratches.

Having by these means determined the bisection at φ, we must bisect the arches F φ and *f* φ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed, that, when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise

it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions. Method of using it for division by continued bisection.

It must be observed too, that, in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\ \delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It is plain that in this way, without some farther precaution, we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the centre of motion d, as the point; but removed from it sideways, by nearly the semidiameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place.

In the operation of bisection, as above described, it may be observed, that, if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger, that the point may be a little deflected from its true course; though in Bird's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would

Method of using it for division by continued bisection.

would be to set the beam compass not so exactly to the true length, as that one scratch would run into the bur of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the centre, and the other from it.

It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to Bird's, namely, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

Method of dividing by quinquesection.

First method. Let a α (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to a, and with the point make the scratch f; then bring the microscope to f, and draw the scratch e; and in the same manner make the scratches d and b. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and, proceeding as before, make the scratches δ, ϵ, and φ. Then the true position of the first quinquesection will be between b and β, distant from β by one fifth of $b\,\beta$; and the second will be distant from δ by two fifths of $d\,\delta$, and so on.

Then, in subdividing these arches, and striking the true divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion, which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, showing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye may be able to place the wire

in

in the proper manner, between the two scratches, without farther assistance; but the most accurate way would be to have a movable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to b, bring the movable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the movable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to b.

It will be a great convenience, that the movable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable errour in the distance of the movable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

This method compared with that by bisection.

To judge of the comparative accuracy of this method with that of bisection, it must be considered, that the arches $\alpha\beta$, $\beta\delta$, &c., though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch $a\,\alpha$ into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\epsilon$, and $\epsilon\,\phi$ are all too long by the small quantity ϵ, and that the arches af, fe, ed, and db are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of the operation; then the errour in the position of $\beta = \epsilon$, and the point b errs $4\,\epsilon$ in the same direction, and therefore the point assumed as the true point of quinquesection will be at the distance of $\frac{3}{5}\epsilon$ from β, and the errour in the position of this point $= \epsilon \times 1\frac{3}{5}$.

By

By the same way of reasoning the errour in the position of the point taken between d and $\delta = \epsilon \times 2\frac{1}{3}$.

In trisecting the errour of each point $= \epsilon \times 1\frac{1}{3}$; and in bisecting the errour $= \epsilon$; and in quadrisecting the errour of the middle point $= 2\epsilon$.

It appears therefore, that in trisecting, the greatest errour we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the errour of the two middle points is $2\frac{2}{3}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection; and the errour of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

2d method of dividing by quinquesection.

Second method. This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page 49, fourth par., at the same distance from the centre of motion as the point is; and this microscope must be furnished with a movable wire and micrometer, as in page 51; and then having first made the fixed wire of this microscope correspond exactly with the point, we must draw the scratches b and β, d and δ, &c., as before, and bring the fixed wire to the true point of quinquesection between b and β, in the manner directed in page 226, and with the point strike the scratch or dot; and if we please we may, for farther security, as soon as this is done, examine, by means of the movable wire, whether this intermediate scratch or dot is well placed.

Advantage of this method.

The advantage of this method is, that when this is done, we may subdivide and cut the true divisions, by making the wire of the microscope bisect the intermediate scratches, instead of being obliged to use the more troublesome operation of placing it in the proper proportion of distance between the two extremes.

Its disadvantage.

This method certainly requires less attention than the former, and the whole seems to be attended with considerably less

less trouble; but it is not quite so exact, as we are liable to the double errour of placing the intermediate point, and of subdividing it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c., at such a distance from each other, that the intermediate one shall be in no danger of running into the bur raised by the extremes.

Third method. Let $a\,\alpha$ (Fig. 3) be the arch to be quinquesected; lay down the arches $a\,b$, $b\,d$, and $d\,e$, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\,\beta$ and $\beta\,\delta$; then, without altering the frame, move the movable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered; but as this method possesses not much, if any advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing farther about it. 3d method of dividing by quinquesection.

It was before said, p. 48, that the centre of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let A δ (Fig. 4) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the centre on which it turns, and M m the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose, that this point is not exactly in the line $d\,\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much one side of $d\,\delta$, as m is on the other, so that if the wire is not perpendicular to $d\,\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason care should be taken to make the wire exactly perpendicular to $d\,\delta$, which is easily examined by observing whether a point appears to run along it, while the beam compass is turned a little on its centre. It is also necessary Farther general observations.

cessary to take care, that the point δ is in the arc of the circle, while the bisection is observed by the microscope, which may most conveniently be obtained, by placing a stop on the support on which that end of the beam compass rests. If proper care, however, is taken in placing the wire perpendicular, no great nicety is required either in this or in the position of *d*.

Another thing to be attended to, in making the wire bisect two scratches, is to take care that it bisects them in the part where they cut the circle; for as the wire is not perpendicular to the circle, except in very small arches, it is plain, that if it bisects the scratches at the circle, it will not bisect them at a distance from it.

There are many particulars in which my description of the apparatus to be employed will appear incomplete; but as there is nothing in it which seems attended with difficulty, I thought it best not to enter farther into particulars, than was necessary to explain the principle, and to leave the rest to any artist who may choose to try it.

It is difficult to form a proper judgment of the conveniencies or inconveniences of this method, without experience; but, as far as I can judge, it must have much advantage, both in point of accuracy and ease, over that of dividing by the common beam compasses; but it very likely may be thought, that Mr. Troughton's method is better than either. Whether it is or is not, must be left for determination to experience and the judgment of artists. Thus much, however may be observed, that this, as well as his, is free from the difficulty and inaccuracy of setting the point of a compass exactly in the centre of a division. It also requires much less apparatus than his, and is free from any danger of errour, from the slipping or irregularity in the motion of a rolle; in which respect this method, notwithstanding the precautions used by him, is perhaps not entirely free from objection; and what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errours, and in adjusting a sector according to the numbers of that table.

Mr. Troughton's method.

XI.

XI.

Description of a Machine for securing Persons attempting Depredations, without affecting their Lives or Limbs. By Mr. ROBERT SALMON, *of Woburn**.

SIR,

I Beg leave to submit to the Society of Arts &c. a Man-trap, which I hope will meet with their approbation. To those who live in the country it is needless to explain the frequency of petty depredations committed on gardens, orchards, &c., which are sometimes very vexatious. Few persons would like to endanger the life or limb of the depredator by setting the common steel man-trap; yet it is presumed there are but few, who would not wish to detect the offender. The instrument which I have the honour to submit to the Society is for the purpose of catching and holding the person without injury. Man-traps useful to check petty thieves, but the common ones dangerous.

At the agricultural meeting at Woburn last summer an ingenious invention for a similar purpose was produced by Sir Theophilus Biddulph; it consisted of a wooden box, containing two springs in iron barrels, and two chains passing over and round them; when this was set, the chains were withdrawn from round the barrels, and extended to a certain distance. A trigger then kept the trap from closing, the whole was then covered over with thin iron plates, so that if a person set his foot on these plates his leg dropped into the box, and the chains closed round it and held the leg; but as the box was about three feet square and a foot deep, it was requisite that it should at setting be let into the ground, which would be a work of considerable labour; and when done it would be difficult to dispose of the stuff from the hole, or to conceal the trap; and as the whole apparatus was cumbersome and expensive, it appeared to me not to be well applicable in practice. Contrivance to secure a thief without injuring him too cumbersome.

* Trans of the Soc. Arts, vol, XXVII, p. 181. The silver medal of the Society was voted to Mr. Salmon for this invention.

I think

Another simple contrivance for the purpose.

I think it right to give this explanation in justice to Sir Theophilus Biddulph, from whom my idea of the utility of something of the kind arose, as also to show the difference between his invention and the trap I have made, which is so very simple as hardly to require explanation. When set it only requires, that the two keys be withdrawn, and that the trap be covered with a few loose leaves or mould. To the trap I have attached a piece of chain and a screw to be screwed into the ground, so as to prevent its being carried away; against any person that may be caught such a precaution is perhaps unnecessary, for any person who is caught will find the jaws of the trap close so fast on the leg, that he cannot drag the trap far without great pain, and will consequently be glad to stand still, and to call out for relief. For the convenience of explanation I have applied mufflers to the jaws of the trap, so that any person may put in his leg without the least inconvenience. I have even tried it without, yet, though void of danger, the sensation is not pleasant. The muffler will of course be omitted when set for use, as it is not then necessary to guard against a little inconvenience, otherwise the springs might be made weaker.

I remain, Sir,

Your most obedient humble servant,

ROBERT SALMON.

A screw contrived for fixing any thing steadily in the ground

P. S. Permit me strongly to recommend to the notice of the Society the earth-screw attached to the trap, as excellent for the purpose of fixing any thing steadily in the earth. This screw is far superior to the common way of driving an iron point or stake therein.

very useful on various occasions.

I have employed it for several years in fixing cross-staves and other surveying instruments with great advantage. The very act of driving a spiked instrument into the earth leaves it loose with some play or movement, which prevents it from being easily secured; but with a screw of this kind at the bottom of the instrument it is firmly fixed in the ground, and a turn of the screw will again fix it, if it should by any means be moved or loosened. It may also be screwed into the ground with any instrument upon it, which would be spoiled by the act of driving it in.

Description

Description of Mr. Salmon's Man-trap, which detains the Offender, without injuring or maiming him. See Plate II, Fig. 1.

The principal figure in the fore-ground of Plate II is a perspective view of this machine. Fig. 1, A B C is a frame of wrought iron, about 18 inches square; it has an eye projecting from it to receive a short chain, the other end of which is fastened to an iron screw, shown separately at D, screwed into the earth by the key or handle E; this screw is about 14 inches long, and, when screwed into hard ground, will hold so firmly, that there is no danger of its being drawn out, even by two or three men, and having a small square end, it cannot be turned without the key or handle E; so that an offender would find it extremely difficult to remove the trap: *e f e g* are two iron frames moving on centres in the frame A B C; these frames have a constant tendency to close together, by means of two springs, *p p*, fixed in the frame A B, and acting against pins projecting from the upright sides of the moveable frame *e e*; *k k* are two small iron rods jointed to the upper rod of the movable frame *g*, and passing through small locks, *l l*, fixed to the other frame *f*. These locks contain clicks which are pressed by springs into the teeth, as may be seen upon the rods *k k*, so as to prevent the two bars *f g* from being drawn asunder when they have been closed by means of the springs *p p*. The internal mechanism of the locks is explained by figures 2, 3, on a larger scale at L M, in the same plate; one side of the lock is supposed to be removed to exhibit its interior parts, where *k* represents the rack, or that part of the rod which is cut into teeth, *r* is the click, which engages the teeth of the rack, and prevents its being drawn through the lock: the click is pressed against the teeth of the rack by a spring, which is plainly seen in the figures; the locks are attached to the ends of the bar *f* of the movable frame, by the bar passing through the locks, and when the lids are rivetted on it is confined in such a manner that it cannot be got out. But as it is necessary to open the bars *f g*, and draw the clicks back from the teeth of the racks, Mr. Salmon has contrived two dif- Description of the trap.

ferent

ferent methods of accomplishing this object. Figure 3, M, is that which is used in the model left at the Society's Repository. A small key or screw S is put down through a hole in the lid of the lock, and is received into a hole lapped with a screw in the click; by turning the screw it lifts the click out of the teeth of the rack; so that the moving frames *f g* can be opened apart from each other, till they lie flat upon the frame A B. The iron cross *m* is then put between the two rods *f g*, the screws S of the two locks are to be withdrawn from the locks, and the trap is set for use. If an offender should place his foot within the square of the frame, he would tread down the cross *m*, and having thus removed the obstruction, the two frames *e f e g* are closed together by the springs *p p*, so that the bars *f g* enclose his leg, and the clicks in the locks prevent the bars being opened without the screws S. In some of the machines which Mr. Salmon has made since the model was deposited with the Society, the locks are made like figure 2, L, where a common key is to be introduced, and, when turned round, catches the tail of the click; it may have wards to prevent the using of a false key, though no wards are shown in the plate.

Screw for fixing any thing in the ground.

Part of the screw D for securing the trap from being carried away by depredators, is shown on a larger scale at N, in order that the peculiar form of its threads may be better seen, which fix it firmly in the earth. Such screws would be very serviceable in fastening horses at grass, &c.

XII.

Method of constructing commodious Houses with Earthen Walls. By Mr. ROBERT SALMON, *of Woburn**.

DEAR SIR,

Building houses with earth.

HAVING for some years past practised at this place the art of pisé, or constructing walls with earth; and having in

* Trans. of the Soc. Arts, vol. XXVII, p. 185. Twenty guineas were voted to Mr. Salmon for this communication.

consequence

consequence been several times, both publicly and privately, called on to communicate my observations thereon; I have been led to consider, that the best mode of generally communicating what I know on the subject would be through the medium of the Society of Arts &c. I have accordingly, by the waggon, forwarded a case containing a model of my frames and apparatus for performing the work, with every particular in my power to give, for the information of any persons inclined to build in that way; and they will, I hope, be found worthy a place in the collection of the Society.

To such as may be inclined to see specimens of this work, and may not have an opportunity of going far distant from London, I can recommend a house and other works built, and some of them inhabited by my brother, Mr. William Salmon, Builder, at Henley Hill, near Barnet, Herts. Some near London.

I have the honour to be,

The Society's and your most obedient servant,

ROBERT SALMON.

Description of the Engraving of Mr. Salmon's Method of building Pisè or Earthen Walls.

Fig. 4 of plate II is a perspective view of the apparatus or moulds, in which the earths are rammed to form a wall. The mould consists of two long planks F *f*, twelve feet long, twenty inches broad, and one inch thick, each made in two breadths; they are strengthened by several pieces of wood nailed across them. Holes are made through these pieces of wood at top and bottom, to receive iron bolts, which hold the two boards parallel to each other, fourteen or sixteen inches asunder, which is the thickness of the wall intended to be formed between them. The bolts have a large head at one end, and a key passes through the other, to keep the planks together. When a wall is to be built, the foundation is laid in brickwork, which is carried about nine inches above the ground, upon this brickwork the planks are placed and bolted together. Two boards, like that shown at G, are placed between the planks at the ends, to form the ends of the mould; these boards are placed between the two bolts Explanation of the plate.

Explanation of the plate. *a a*, which are seen close together at the end of the moulds, and are held fast by that means; the earth is now to be rammed in between the moulds, by the rammer with an iron head X. When the mould is filled with earth and well rammed down, the keys are to be taken out of the bolts, and the bolts drawn out; the planks are then removed, and put together again, a length farther upon the wall, the bolts at the end being put through the holes left in the wall. Only one of the end boards is now put in, and the ramming proceeds as before. In this manner straight walls may be built of any length, and when the lower course is finished, then the mould may be taken to pieces, and put together again upon that course; the lower bolts of the frame being put through the bolt holes, which the upper bolts made in the wall at the first operation, to insure that the upper part of the wall is in the same place, and exactly over the lower. When a wall is to be built thinner than usual, a block of wood must be placed under the head of each bolt, so as to diminish the space between the planks.

When the angle walls of buildings are to be made, the apparatus is put together, as shown in the plate; four of the planks are put together to form a right-angled mould; one end of each of the planks F and H is furnished with double bolts, the other ends have each two eyebolts fixed into them, as shown separately at *b d*; then a bolt *n* connects the two moulds, so as to form a hinge; the planks are kept together so as to be perpendicular to each other, by a long iron rod K, hooked into eyebolts fixed in the planks. The outside planks of the mould are joined together in a different manner, see fig. 5, that of one frame being longer than that of the other, and has two pair of holes through its end O, to receive the bolts *t t*, which are fastened to the ends of the other shorter plank, and the keys are put through the ends of the bolts, to secure the planks together; a piece of wood P is occasionally placed between the end of the short plank and the side of the other, to increase the space between the planks, to make a thicker wall, the two bolts at the end of the plank being received into the notches in the piece of wood, and these bolts are then put through the holes Z Z of the long plank. In building the angle wall, it is necessary, that

that the vertical joints formed between each mould should not be over one another, but arranged in the same manner as the joints of brickwork; this is accomplished by making the lower course of wall upon the brickwork only half the length of the mould, which is done by placing the end board G of the mould in the middle of it. The next course over this is to be made the whole length of the mould, the next one only half, and so on, as shown in the figure.

The model of the frame in possession of the Society is made to a scale of an inch to a foot. The frame at large is made of $1\frac{1}{2}$ inch deal, ploughed and tongued together; the bolts and pins or keys of iron, as are also the plates on the holes in the sides of the frame. These plates are put to prevent the keys from cutting into the wood, and the holes from gulling and wearing.

Method of building earthen walls.

This sort of mould is calculated for making walls either fourteen or sixteen inches thick, and the model (or perspective view of it in the distance of plate II) shows how the mould is to be applied for making the corner of a building of the sixteen inch wall; the same moulds may be applied for a fourteen inch wall, *ff* being the outer sides, F H the inner sides. When employed for straight walls, or making good between the corners of buildings, the two returns of the frames are used in pairs, *ff* and F H make two sets of frames. The board marked G must be of width equal to the thickness of the walls to be made, and is for the purpose of stopping the earth, and making ends or jambs to doors or windows, or wherever wanted. The piece of wood P is two inches thick, and is for the purpose of making out the external sides of the moulds, from a fourteen inch to a sixteen inch wall: by introducing this piece between the two sides *ff*, and putting the fixed iron pins in the outer holes Z Z, and taking away the blocks under the heads of the outer bolts, the sides of the frame will then be sixteen inches, as under, and thereby adapted for a sixteen inch wall. Fig. E are pieces of wood about $1\frac{1}{2}$ inch square, and cut to the length of the thickness of the wall, and are for gauges to be applied on the top of the bolt, to keep the keys from drawing the sides too close together.

In

Method of building earthen walls.

In beginning the wall, some of them are necessary at the bottom, the more firmly to support the frame on the brick or stone work. They are then worked into the wall, and, after the frame is taken down, driven out. After the first course, they are only necessary to the top irons, and may be taken out as soon as the earth is rammed up near them, so that no holes are left in the upper courses of the wall, more than the bolt holes.

When these frames are used, one side is placed in such a direction, that the front or end may be required to be taken away, and then by means of the angular iron brace K, the other return is sure to stand at right angles with the first. Care should then be taken, in the first course, to set the sides level, that being done, the other upper courses, from the nature of the frames, and manner of using them, must of course come upright and level without any particular care, and a wall properly begun cannot well get wrong. After the first course of a building is done, the moulds should be moved to another, and so on till all the courses are up, and as the top holes of each preceding course become the bottom holes in the succeeding ones, no difficulty will be found in fixing the mould after the first course is properly done.

Fig. 6 shows the iron pin and staples that keep the internal angle of the frame together. K, fig. 4, is an iron stay to set the returns at right angles. This is only wanted where other means of setting the building square is not to be obtained.

Having described the frame, and means of applying it generally, it may be necessary to observe the following particulars in the process. Having carried one course round the building, it frequently happens that the top thereof becomes too dry to attach to the next succeeding course; and therefore it is advisable, that, as soon as the frame is set for the succeeding course, a small quantity of thick grout, composed of $\frac{1}{5}$ lime, and $\frac{4}{5}$ earth, be poured on the top of each course, immediately before the first layer of earth is put in. A very small quantity is sufficient, and will add much to the strength of the work, by cementing the cours s well together at the joints. The workman should also, with the

the corner of his rammer, in ramming home to the upright joints, cut down a little of that part of the wall, up to which he works; this will make the upright joints key together, and unite in a solid manner. Having thus proceeded and got up the walls, the next thing will be to stop the bolt holes, with mortar made ¼ lime and ¾ earth the same as the wall.

The earth proper for this work should be neither sand nor clay, but partaking of both. Clay is particularly objectionable, as is also chalk, or calcareous earth of any sort. Sand is also not proper, unless accompanied with some binding quality: the bolder and coarser the sort of earth the better. When used, it should retain no more moisture than just to make it adhere together, under the pressure of the thumb and finger. Notwithstanding earths bordering on sand appear to make the strongest work, nevertheless good earths may often be found in parts that do not abound with sand. Those that abound with a mixture of grit or fine gravel are generally the best. Having provided proper earth, as much should be put in each layer as to form about an inch and half when compressed by ramming. Earth proper for the purpose.

The rammer X should not be more than half an inch wide on the edge, in order that it may more forcibly compress every part of the earth, which a flat rammer would not do so well. The rammer.

In making the walls, about three inches in thickness of loose earth should be put in each course, which done, the same, by means of a trowel made for the purpose, is drawn back and cleared from the face of the wall, and the space then filled up with the facing composition, forming on an average about one inch in thickness; the whole then is firmly rammed, (in which, and properly preparing the facing stuff, much depends the perfection of the work,) till it is quite hard, when it will be compressed to about one inch and a half in thickness. The common facing stuff is composed of lime one part, and earth, the same sort as used for walling, three parts. The lime and earth mixed and slacked together, the same as for mortar. The more it is slacked and wetted the better, provided time can be allowed for it again to dry and pulverize, so as to be fit for ramming. The Composition of the wall.

better

better sort of facing stuff may have a small quantity more of lime in it.

Foundation. The foundation should be of brick or stone, carried up nine inches above the ground; and if a plinth is to be shown, then one course above the same should be of brick or stone, to prevent the water that might lodge on the plinth from damaging the earth wall.

Season for building. The proper season for performing this work is any time that the earth is to be procured sufficiently dry for the purpose; the more early in the season the better, in order to give it time to dry before finishing, or if late it would be advisable not to finish till the year after it is built.

Windows and doors. Windows and doors may be left in the walls wherever wanted, by fixing the head of the moulds and carrying up quoins to form the same: in erecting which some bond timber should be laid in coarse mortar and rammed in with the earth. Lintels may also be laid at the proper height. This method is cheapest, where only one window or door of a size is wanted, but if many, the readiest way would be to make some rough frames of boards of width equal to the thickness of the walls, and place them in the situation of the windows and doors. When done the earth is rammed up to them, laying bond timber at the sides and lintels over them. In both cases the windows and door frames are to be put in their places, and fastened to the bond timber, after the wall is up. The bond timber, lintel, and plates, should be kept as thin as possible, in order to prevent any disagreement between the earth and timber in the shrinking or drying of the same. The bond timber about 4 inches by 1½; floor or wall plates 6 inches by 2; lintels about 4 inches thick; and it may be worthy of notice, that any slabs or rough stuff may be used, the earth being sure to ram close to it and keep it in place.

Finishing of ordinary work. For common cottages, when the whole of the walls are up and covered in, the holes should be stopped with very coarse mortar, made the same as the facing stuff, but used wetter, and the wall then lime-washed over with lime and sharp sand, which should be made up in small quantites and used while hot. This may readily be done by adding a knob of lime and sand a little at a time as it is used.

For

For better kind of cottages the better sort of facing stuff may be used, and then, as before, the whole lime-whited; or if it be required to make the finishing as perfect as possible, the following is the best mode, viz. with water and a brush thoroughly wet and soak the face of the wall for two or three yards in superficies at a time; all which part, during the said wetting, should be continually rubbed and worked about with a hand float, till such time the face is rubbed smooth and even, by which the facing composition will so wash up as to become a pleasant regular colour, the face smooth and hard when dry, and not liable to scale off as a coat of plastering would do. This finishing will be still improved by a small quantity of lime being put into the water used for soaking the face: and if, after the wall is well soaked and rubbed, as abovementioned, there be thrown thereon with a brush some lime and sand, (such as is used for lime-whiting,) and this also worked into the face; the face will then become as perfect and hard as stucco. Finishing of better work.

Having explained the frames as constructed by me for performing earth walling, as also the manner of finishing it, I beg leave to lay before the Society some observations on these, compared with the original French means and manner of performing the same, as described in the first volume of Communications to the Board of Agriculture.

The sides of the frames, as formerly constructed, were supported on joists or cross pieces of timber, which pieces were cut into the top of each course of walling. The sides were then kept together by upright timbers framed into the cross pieces or joists, and the tops of the upright pieces were wristed and held together by ropes going across the frame from one side to the other. In consequence of this construction, by experience I found much labour was lost in cutting the channels to lay the cross pieces in. These channels, after the buildings were up, took labour and materials to fill them in, and rendered the walls less strong. Also the difficulty of getting the frame rightly placed every time it was moved, and the elasticity of the rope across the top, made the whole very imperfect, so much so that all work done in that manner was untrue and unsound; as the rope, however tight it might be strained, would yield to a certain Original French mode. Its inconveniences.

degree. The labour of moving was great, and when the frames were set, the cross ropes and uprights above the sides were much in the way of the workmen.

Advantages of the new mode. On examining the model I have the honour to send, it may be seen, that these frames being once set true, they require very little care afterward: being kept together by iron bolts no elasticity can occur, and the earth will be as firmly compressed, as if rammed between two walls. No cutting away for cross pieces is required, nor any holes but the small bolt holes to make good; and as nothing sticks up above the frames, the workman cannot be impeded. In consequence of these alterations the work may be more cheaply and truly executed than with the old sort of frame.

Previously to entering into the expense of this sort of work, or my conceptions as to its advantage, it may be necessary briefly to state whence such are collected.

Trial of the old mode, About sixteen or eighteen years ago, the late Duke of Bedford directed a foreigner, who was then making some walls in Lancashire, to come and make some specimens here; and wishing to know how far it might be usefully introduced, I was directed to give attention, and every aid, to the man employed. Accordingly frames of the old sort were made, exactly like those before described, and with them some specimens being made, the man returned. These specimens I considered were very bad walling, and in attending to the execution thereof, seeing sufficient room for improvement, and of the new. I was directed further to practice it. Frames were then constructed like the model, and several walls erected, among which were some cottages now standing, and lastly, the house I now live in. This has been built about twelve years, and is a sufficient proof of the utility of the practice: the house being as close, warm, and dry in the walls, as if built of any materials whatever.

Expense. With regard to the expense of the walls of this sort, as labour is the principal part of the expense, and as in some places labour is dearer than in others, the best mode of estimating it at different places will be from the quantity that a man should do in a day, and which I have found to

be

be $1\frac{1}{4}$ yard superficial, in the common day's labour of ten hours.

At this place the expense may be estimated as follows:

	£	s.	d.
Labour to making facing composition, fitting in and ramming to a 16-inch wall, where the earth is at hand (labourer's wages being 1*s.* 10*d.* per day) per yard superficial	0	2	2
Value of lime used in the composition rammed into the face of a yard superficial (lime being 8*d.* per bushel)	0	0	3
Lime and labour to rubbing up and finishing the outside face of the wall	0	0	3
Total finished and faced on one side	0	2	8
If a wall to a garden or otherways, and finished and faced on both sides, then add	0	0	8
Total for walls finished on both sides	0	3	4

At this place the value of a yard of brick-work is more than ten shillings, of walling only 14 inches thick, the bricks being 42*s.* per 1000, and lime 8*d.* per bushel; consequently the œconomy of the pisè must appear; and the same difference will be found in any other place where lime and bricks bear the same price, and proper earth can be found at hand. But as attempting this sort of work, where it is not applicable, or improperly doing it, so as to lead to failure, may prevent its introduction where it would be useful, I shall endeavour to point out any precautions that have struck me, and every thing that has appeared to make against it.

Many persons have supposed, and it has been asserted, that almost any earth will do, but such is certainly very erroneous; for proper earth cannot in all places be found, and it being difficult to describe it, or to be sure when it is found, it seems advisable, before the entering on any considerable work, that the experimentalist should first do a small piece, All earth will not do.

 piece,

Previous trial recommended.

piece, and let it stand with the top only covered for a winter at least.

Requires care,

It has before been observed, that the excellence of the work depends on its having due compression, as well as being of proper soil. If the compression be not perfect, although the soil be good, the walls will be unsound; and unfortunately it so happens, that when a wall is built and badly rammed, its imperfection cannot readily be observed; and farther, the defect is likely only to be found but by its failure. Hence arises the greatest bar to its general introduction; for, as it requires considerable labour to build a wall, it requires exertion to do it in proper season; and if the labourer be employed to do the work by task, it becomes his interest to get on and do it slightly, and if done by day, it will not advance so rapidly; consequently, in either way, it will require great attention from a careful overlooker.

and attentive overlooking.

The saving in expense solely in the walls.

From the foregoing comparative statement of pisè against brickwork, persons unacquainted with building are inclined to suppose, that the whole expense of the building will be in proportion thereto, contrary to this it only effects the walling,—the roof, floor, &c., remaining the same as before, excepting as it may reduce the quantity of bond timber and lime used in plastering the inside, this latter is less than when plastered on brickwork, the face of the wall being so much truer than brickwork.

A working drawing, on a scale of one inch to a foot, is left with the Society, for the inspection of any person inclined to construct the apparatus.

XIII.

Curious Property of the Toád. Communicated by a Correspondent.

To Mr. NICHOLSON.

SIR,

AMONG the many curious accounts that I have read of the Toad, I never met with the following fact.

A person

A person in the neighbourhood of Maidstone, who manufactures brown paper, informed me, last summer, while I was observing his people at work, that he had frequently placed a toad in the midst of the pile of sheets to be pressed, and always found it alive and well on taking it out, although it must have sustained, with the paper, a pressure equivalent to several tons; but a frog could never survive the same degree of pressure. I searched a long time for a toad, to see the experiment myself; but it was a very warm day, and I was unable to find one, till after the men had left work.

The toad capable of sustaining very great pressure.

I am, Sir,
Your obliged,

March 25th, 1810. A. M.

XIV.

Communications on the Mode of Action of the Galvanic Pile announced. By J. A. De Luc, *Esq. F.R.S. &c.*

To Mr. NICHOLSON.

SIR,

YOUR kindness in admitting into your celebrated Journal some former papers of mine induces me to offer you some others; as my view to bring to the knowledge of natural philosophers my discoveries concerning the *Mode of Action* of the *Galvanic Pile* will be as effectually accomplished by that channel, as through the Phil. Trans. of the Royal Society, into which (I know not why) they have not been admitted. The following is a short account of what has passed on this occasion.

Discoveries concerning the action of the galvanic pile announced.

In January 1806, I had the honour of presenting to the Royal Society two works, connected with each other, under the titles of, *Introduction à la Phys. terrestre par les Fluides expansibles*, and *Traité élémentaire sur le Fluide electro-galvanique*, which works I wrote at Berlin during my stay in that place, and which were printed at Paris; the latter of them contains a Process of Analysis of the Galvanic Pile, which

Two works on natural philosophy presented to the Royal Society,

which, on account of my design to make geological observations in various parts of Germany, I had left unfinished, in hopes that this process would be followed by some experimental philosopher; which however has not been the case.

and a paper on galvanism.

On my return to Windsor, where I had resumed this rich analysis, I saw, in Part I of the Phil. Trans. for 1807, a Bakerian Lecture of Mr. Davy, *On some Chemical Agencies of Electricity*, read the 20th of Nov. 1806; in which, intermixed with very ingenious experiments and important discoveries in practical chemistry, I found some principles laid down as foundations of a theory, which had already been opposed by the experiments related in the last of the abovementioned works, deposited in the Royal Society's library the beginning of the same year. This engaged me to follow more closely the analysis which I had resumed, and, having completed it, I wrote a paper on this subject, which was presented to the Royal Society the 30th of May, 1808.

This abridged.

Having heard that this paper had not been read, and being told, that it was on account of its length, I took an opportunity of seeing Sir Jos. Banks at Windsor in the beginning of 1809, to beg of him, that this paper should be sent back to me, in order that I might contract it, which he did very obligingly. One of the abbreviations that I adopted was to suppress the application of my experiments to Mr. Davy's theory; and by this and other omissions, having reduced my paper to twenty-three pages, I sent it again to the Royal Society the 25th of Feb. 1809.

Chemical effects of the pile separable from the electrical. New meteorological instrument.

One of the important results of the experiments contained in this paper is, that, in the *Galvanic Pile*, the *chemical* effects can be separated from the *electrical*; and that the latter lead to a new *meteorological* instrument, very desirable for the knowledge of atmospheric phenomena. This object I followed subsequently to the first presentation of my paper, and on the 7th of March 1809, I presented to the Royal Society another paper under this title: *On the Electric Column, and Aerial Electroscope.*

Publication of the papers deferred by the Royal Society

The following summer, I received a letter, dated from *the Apartments of the Royal Society* the 6th of July, signed Humphry Davy, S. R. S. saying: *that the Committee of Papers, although they did not think it proper to publish my papers*

papers at present, had directed that they be deposited in the Archives of the Society. And at the same time I was informed, that this had been decided from the minutes only of the Secretary Dr. Wollaston, which certainly could not give a sufficient knowledge of papers consisting of descriptions of experiments and of immediate results. Therefore, at the next meeting of the Royal Society, I sent to the President a petition to the Committee, dated the 5th of Nov. 1809, in which, founded on the above circumstance, I claimed a revisal of their decision, by judging of the papers themselves. But soon after this petition was sent, I was assured, that, whatever might be my reasons for the claim of a revisal, I should not obtain it, the Committee never changing their first resolution; and this proved to be the case, as I was informed on the 25th of December. I wrote the 28th, begging that the drawings annexed to these papers should be lent me, in order to have them engraven, as I meant to publish my papers myself; but to this day I remain without an answer.

Electric column.

In the mean time I had made no secret of my discoveries. I had communicated the *electric column* and its construction to many experienced philosophers, and in particular, being in London in July 1808, I showed it to Mr. Davy. From this, and the reading the minutes both in the Royal Society and in the Committee, the construction and phenomena of the *electric column* are much known, and it has been executed by many experienced philosophers. Thus, a publication of this instrument by me will at present be deprived of the merit of novelty; but only with respect to its immediate and surprising phenomena: for the chief importance of the *electric column* is its connection with the *galvanic pile*, hitherto consigned only to my detained papers and to the memory of those of my friends, to whom I have explained it.

The subject promised in a more minute state.

In proposing to you, Sir, to be the channel through which natural philosophers at large may be informed of these discoveries, I do not intend to offer you the publication of these papers, as they were sent to the Royal Society; for they had been composed during the progress of my experiments, which produced a form at present not so convenient. The progress of discoveries is always interesting in

the history of science; and with respect to this object, my papers, deposited in the archives of the Society, may certify the date of the steps; which however were not completed, for since that time I have carried them farther. Therefore, for your Journal, if you accept my offer, I shall take the subject as it now stands, dividing it under three heads: *Analysis of the Galvanic Pile.—Production of an Instrument giving spontaneous signs of Electricity, without chemical Effects.—Analysis of the Electric Machine, compared with that Instrument.*

If you are disposed, Sir, to undertake this publication, I shall not have recourse to any other method of giving these experiments to the public. You may insert this letter in your next number, in order to announce the subject; for the following number, you shall have the first of the above papers, and the others in monthly succession. To each paper a drawing will be annexed, but I shall make these as simple as possible.

I shall be very happy to have with you this new connexion, and I remain, Sir,

Your obedient humble servant,

Windsor, DE LUC.

22d March, 1810.

Annotation.—This letter is inserted at the request of the learned author, to whom I have answered, that I shall be happy to pay every attention to his communications, which my duty as a Journalist, and the nature and importance of those writings, may demand. W. N.

XV.

Observations on Galvanic Batteries. In a Letter from Mr. CHARLES SYLVESTER.

To Mr. NICHOLSON.

DEAR SIR, *Derby, April 17th,* 1810.

Facts in the article Galvanism in Nich. Enc.

THE article GALVANISM, which I had the honour to write for the Encyclopœdia lately published by you, contains

tains some facts which had not before appeared in print, and, consequently, were not backed by any specified authority. As the authenticity of them might on this account be questioned, I beg to state, through the medium of your Journal, that they were developed during a series of experiments, in which I was engaged several years ago, and have been very frequently exhibited in my lectures upon the subject. In my present communication, I shall take the opportunity of recording one or two others, which did not occur to me at the time I wrote that article, and which, in a practical point of view, may not be deemed altogether unimportant.

Observed by Mr. Sylvester.

About three years since, I devoted some time to the investigation of the subject of galvanic batteries, and have now in my possession one made at that period; the cells of which are formed with glass plates, so as to allow the plates of metal to be taken out and put in at pleasure. A similar plan is at present in use, and these batteries are said to possess the advantage of exposing double the surface of the common ones. I did not find this the case by any means, in my inquiry into the subject, and it was under this idea, that I did not persevere in the discovery. The reason I give for these batteries not having double power is, that two of the surfaces of each pair of plates are not contiguous. I have since that time obviated this evil, and hope very soon to give you an account of a battery, considerably more improved in construction. A fact, which furnishes a useful lesson upon this point, is the following: let two wine glasses be nearly filled with dilute muriatic acid; and afterward immerse in them an arc composed of a piece of zinc and a piece of copper wire; the zinc being in one vessel, and the copper in the other. If another arc, formed of similar wires, be then plunged into the glasses, in a reversed order, observing that the arcs do not touch each other, a very rapid decomposition will take place, and hidrogen will be evolved from each of the copper wires. Let one of the above arcs be now removed, and the connexion be made by means of an arc consisting of one metal only; the copper wire of the *compound* arc will be seen still to afford bubbles of hidrogen, but in much less quantity. Instead of this *simple* arc, employ

Battery with the metals separated by glass plates

has not double the power of the common ones.

Fact apparently proving, that the copper surface should be greater than the zinc.

ploy next a slip of copper of a conical shape; and on reversing it, a very striking difference will be observed. When the small end of it is placed in the glass containing the zinc wire, no greater effect will be apparent, if it present nearly the same surface, than when the copper wire was used as the connecter; but, when the broad end occupies the same situation, the effect in decomposing water will be found increased to a very considerable degree. This experiment appears to prove, that the copper surface, in galvanic batteries, ought to be greater than the zinc surface, in order to produce the greatest possible effect.

Effects of a galvanic battery counteracted by a coating of mercury in the metal,

I shall conclude my present letter by mentioning a curious anomaly with respect to the agency of mercury in galvanic combinations, which I do not recollect to have before seen noticed. For the purpose of showing the decomposition of water by galvanism, I have been accustomed to employ wires of various metals connected with zinc, using dilute muriatic acid as the oxidating medium; and have found silver, copper, bismuth, cobalt, arsenic, as well as many others, answer the purpose extremely well. In making the experiment at one of my lectures some time ago, however, I was surprised to find, that the usual effects were not produced, and was very long at a loss to conceive, what could be the cause of so curious a circumstance. By an attentive examination of the phenomena, I perceived, that the wire which ought to have furnished the hidrogen had become coated with a white matter, which, on subsequent research, proved to be mercury, and had been obtained from the reduction of corrosive sublimate, which entered, as an impurity, into the composition of the muriatic acid I had employed. Since that period, the accident has happened to me twice, in distant places, from the same cause; a fact, which seems to point out corrosive sublimate as a more frequent ingredient in marine acid, than might be at first suspected. This experiment contributes to the support of an opinion, that galvanic effects are not entirely dependent upon the relative oxidability of the metals employed, as has been hitherto generally supposed; for although, in the above case, the quicksilver was nearly as oxidable as the metal

which arose from the muriatic acid being contaminated by corrosive sublimate.

This impurity more common than suspected.

metal which it coated, yet the activity of the galvanic influence was interrupted.

I am, Sir,

Your obedient servant,

CHARLES SYLVESTER.

XVI.

Remarks on Professor Leslie's Doctrine of radiant Heat; with Experiments to show, that Caloric can pass through transparent Media without heating them. By J. D. MAYCOCK, *Esq.*

To Mr. NICHOLSON.

SIR,

ALTHOUGH the most approved systematical writers on chemistry have not adopted the opinions of Mr. Leslie, respecting the manner in which an equilibrium of temperature is preserved among distant bodies, they have conceived the result of his experiments sufficient to prove, that dense media are impervious to caloric radiated from surfaces artificially heated.

Dense media supposed impervious to radiating heat,

I shall not enter into a minute examination of the merits of the hypothesis, proposed in the "Experimental Inquiry," for explaining what is commonly termed the radiation of caloric; but shall content myself with reminding you, that it rests entirely on a supposition, that caloric passes through no medium without first raising its temperature: the truth of which Mr. Leslie considers as proved by the effects of different skreens interposed, in his experiments, between the thermometer and the source of heat. But as in the experiments alluded to no regard was paid to time, I hold all reasonings founded on them as fallacious and unsatisfactory; and having myself obtained results, which favour opinions very opposite to those advanced by the learned professor, I take the liberty of communicating them to you.

and caloric to raise the temperature of every medium it passes through.

Exp. 1. I placed two metallic reflectors, 12 inches diameter, and 5½ in. focal distance, fronting each other, and 30 in. distant; I brought a differential thermometer into the

Experiments to prove the contrary.

the focus of one, a lighted candle into the focus of the other reflector. In two minutes the thermometer fluid rose 63°. This I determined by taking the mean of six experiments, the results of which, from attention to the burning of the candle, were very nearly alike.

I procured three skreens; one of painted glass, one of painted tin (both black), the other of plain glass. They served for the following experiments.

Exp. 2. *Exp.* 2. I placed a skreen stand midway between the two reflectors. On this I fixed the painted glass skreen; in two minutes the thermometer rose 2°: repeated, 2°: repeated, $1\frac{2}{3}$°.

Exp. 3. *Exp.* 3. I exchanged the painted glass for the painted tin. In two minutes the thermometer rose $1\frac{3}{4}$°: repeated, $1\frac{2}{3}$°. repeated, $1\frac{2}{3}$°.

Exp. 4. *Exp.* 4. Having removed the painted tin, and interposed the plain glass, the thermometer rose in two minutes 25°: repeated, 26°: repeated, $25\frac{2}{3}$°.

Exp. 5. *Exp.* 5. The apparatus remaining as in the preceding experiments, and the glass skreen resting on the stand, when the thermometer had fallen to zero, I brought the lighted candle into the focus of the first reflector; the thermometric fluid mounted in two minutes to 24°. I removed the candle, but left the skreen. The thermometer did not rise in the slightest degree, and in 30″ it had fallen 3°; in 45″, 8°; in 60″, 12°; in 75″, 14°; in 90″, 16°; in 105″, 18°; and in 2′, 19°.

Exp. 6. *Exp.* 6. I substituted the painted for the plain glass. In two minutes the thermometer was at $1\frac{1}{4}$°. I removed the candle, the skreen remaining on the stand; in one minute the thermometer rose $1\frac{1}{2}$° more, and after a short time it began to fall.

Exp. 7. *Exp.* 7. Having fixed the blackened glass skreen on the stand, I brought into the focus of the first reflector a 7 oz. flask, containing $4\frac{1}{2}$ oz. of a boiling mixture of sulphuric acid and water, the boiling point of which was somewhere between 450° and 500° Fah. In two minutes the thermometer rose $1\frac{3}{4}$°. I removed the flask—the thermometer continued to rise until at $1\frac{3}{4}$° of the scale.

Exp. 8. *Exp.* 8. I interposed the plain instead of the painted glass

glass. In two minutes the thermometer was up to 2½°. I removed the hot flask; the thermometer did not experience any farther rise, and in two minutes it had fallen ½°.

Exp. 9. A 7 oz. flask of boiling water being used as the source of heat, I found, that plain glass transmitted caloric as 7 to painted glass as 5; and that, after the hot flask was removed, the effect of the black glass skreen was to that of the plain glass as 15 to 13. Exp. 9.

Exp. 10. Having painted one side of a pane of glass black, I placed it on the skreen stand with its blackened surface next to the candle. In two minutes the thermometer rose 2·3°. Exp. 10.

Exp. 11. I opposed the clean surface of the same skreen to the hot body. In two minutes the thermometer stood at 3·8°. Exp. 11.

Exp. 12. I removed one of the reflectors, and found, that, without the skreen was very near to the source of heat, no sensible effect was produced on the thermometer in two minutes. The skreen stand being situate one inch from the hot body, glass did not appear to transmit caloric better than any other medium. Exp. 12.

Before I attempt to draw any conclusions from the preceding experiments, I think it necessary to anticipate some objections, which may possibly be urged to the manner, in which the most striking were made. Objections answered.

The inequality of heat produced by the combustion of a candle may be thought a very fertile source of errour: and I am ready to allow, that the flame of a candle could not with propriety be employed to determine minute differences in the temperature of the thermometer. But the differences I observed were very far from being minute, and the experiments, as often as repeated, afforded similar results. Inequality of the heat of a candle.

The burning candle will be deemed particularly objectionable by all those who contend, that light itself is possessed of a heating property. It would be entirely foreign to my present purpose, to attempt a refutation of this purely hypothetical opinion. I would however avail myself of the excellent experiments of Dr. Herschel, which show, that the heat excited by the different prismatic rays is in no degree That light is supposed to be heating,

gree proportioned to their illuminating power, the greatest effect on the thermometer being produced half an inch beyond the visible spectrum. Indeed Dr. Herschel's several papers, published in the Ph. Trans. for 1800, render it, at least, extremely probable, that the colorific rays themselves do not affect the temperature of bodies. Now it is matter of little consequence, whether we term the invisible heat-making rays light or caloric. If we are pleased to call them light, we must yet admit, that they are invisible; and cannot therefore, because the candle's flame is luminous, object to its being used as a source of heat in experiments on radiant caloric.

or may be thought to render bodies more pervious to heat.

It may still be urged, that, although light, strictly so called, is not possessed of a heating property, yet its association with caloric may cause this agent to pass through media, which under other circumstances would be impervious to it. It is only possible to conceive the transmission of caloric through dense media to be facilitated by its association with light, by supposing an attraction to be exerted by these fluids for each other. But all the phenomena of phosphorescence show, that the very opposite to attraction, namely repulsion, prevails between light and caloric. All objections, however, to the source of heat will be removed, when we consider, that the experiments in which the flask with a boiling mixture of sulphuric acid and water was used afforded results similar to those obtained from the combustion of a candle.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineralogy of Srontian

AT the meeting of this Society on the 10th of March, the Rev. Dr. Macknight read a paper on the mineralogy of Strontian and Ben Nevis. The rocks which compose the districts of Strontian are mica slate, gneiss, and granite; and the lead-glance, which occurs in gneiss, is associated with iron-pyrites, cross-stone, calc-spar, foliated zeolite, strontian, and heavy-spar. Ben Nevis is an overlying massive formation, which rests on gneiss and mica-slate, approaching in some places to clay-slate. In this formation, compact feldspar is the leading ingredient. The inferior mass consists of sienite, passing from the simple-granular to the

and Ben Nevis.

the granular-porphyritic; and the upper portion of the mountain, comprehending the summit, with about 1400 feet of the perpendicular hieght below it, is composed of a dark-coloured rock, which for the most part is porphyritic, and seems to be intimately allied in its characters to compact feldspar. This appears from the gradual transition of the one substance into the other, which is distinctly observed under the tremendous precipice of Ben Nevis to the N. E., and demonstrates the identity and continuity of the whole formation. The colouring matter appears to be hornblende intimately mixed with the substance of the rock. At first view the whole might have been considered as a formation of clink stone and porphyry-slate. But a more minute investigation discovers many oryctognostic characters of distinction from these substances, which are less crystalline, and belong to a more recent era of formation.

Zetland sheep.

At the same meeting, Dr. Arthur Edmonstone read an account of the peculiarities of the Zetland sheep, with remarks on their diseases. And the Secretary read a communication from Lieut. Col. Imrie, describing a vein of greenstone, which occurs in Glenco, and which appears to have been overlooked in the mineralogical descriptions of that district.

Vein of greenstone in Glenco.

Literary and philosophical society at Hackney.

The establishment of societies for the promotion and diffusion of knowledge by reading and conversation, wherever circumstances admit them, cannot fail to promote the general interests of science, and I am happy therefore to announce the formation of a Literary and Philosophical Society at Hackney. The object of this society is not confined to the purchase of books, the use of which the members are allowed at their own houses; but its plan extends to procuring philosophical instruments, and patronising lectures on philosophical subjects, as soon as the state of its funds will permit. A weekly meeting is likewise to be held for literary conversations, and reading such papers as the society may be favoured with. The subjects for conversation, or books for the library, comprehend the mathematics, natural philosophy, natural history, chemistry, polite literature, antiquities, history, biography, questions of general law and policy, commerce and the arts; but topics of religion, the practical branches of law and physic, and the politics of the day, are excluded. Its commencement has been very auspicious.

New edition of Dr. Henry's chemistry.

A new edition of Dr. Henry's Chemistry is in the press. As the late grand discoveries of Mr. Davy have wrought extraordinary changes in the face of the science, the present may be considered almost as a new work, from the various alterations that have been necessary, and the great additions required to detail and explain the progress that has been made. It will now extend to two volumes, which are printing with all possible expedition.

METEOROLOGICAL JOURNAL,

For APRIL, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAR. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
28	45°	45°	50°	37°5	29·70	Rain	Rain
29	44·5	45	52	37	29·93	Ditto	Fair
30	43·5	44·5	48	41·5	29·94	Ditto	Cloudy
31	46·5	46	51	41·5	29·76	Ditto	Rain
APR.							
1	45·5	45	49	41	29·45	Rain	Rain
2	44	44·5	48.5	43·5	29·76	Cloudy	Cloudy *
3	49	50	54·5	40	29 64	Rain	Rain †
4	45	43[illegible]	47·5	35·5	29·44	Ditto	Fair
5	42	45	50	41·5	29·76	Fair	Cloudy
6	44	43 5	49	41·5	29·37	Rain	Ditto
7	46	47	53·5	41·5	29·32	Showery	Fair
8	46·5	47	52	43·5	29·49	Fair	Rain
9	47	46	51	42	29·48	Rain	Ditto
10	44	43	45	35	29·53	Ditto	Ditto
11	40	38	42	32	29·67	Showery	Fair ‡
12	34·5	37·5	42·5	31	29·92	Fair	Ditto
13	36	37	41·5	34	29·94	Ditto	Cloudy §
14	39	41	45·5	38	29·90	Cloudy	Fair
15	45	43·5	52	38	29·83	Fair	Ditto
16	44·5	42·5	52·5	37·5	29·56	Rain	Cloudy
17	45	46·5	53	42·5	29 49	Showery	Fair
18	49	54	60	45·5	29·65	Fair	Ditto
19	51	52·5	59·5	45	29 76	Ditto	Ditto
20	51·5	52	57·5	44·5	29·97	Ditto	Ditto
21	51·5	54·5	61	43	30·20	Ditto	Ditto
22	51	55	63	47	30·23	Ditto	Ditto
23	54	55	64·5	46·5	30·21	Ditto	Ditto
24	52·5	53	62·5	45	30·24	Ditto	Ditto
25	52	48	62	42·5	30·19	Ditto	Cloudy ‖
26	51·5	50	59	43	30·14	Ditto	Fair

* Rain at 11, and all the night. † At 11 stars visible.
‡ The day cold with hail and rain.
§ The day and evening cold. Small fall of snow at 2 P. M.
‖ Evening cold.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1810.

ARTICLE I.

On the Volcanoes of Jorullo; by Alexander Humboldt *.

THE grand catastrophe in which this volcanic mountain issued from the earth, and by which the face of a considerable extent of ground was totally altered, was perhaps one of the most extensive physical changes, that the history of our globe exhibits. Geology points out spots in the ocean, where, within the last two thousand years, volcanic islets have arisen above the surface of the sea, as near the Azores, in the Archipelago, and on the south of Iceland: but it records no instance of a mountain of scoriæ and ashes, 517 met. [563 yards] above the old level of the neighbouring plains, suddenly formed in the centre of a thousand small burning cones, thirty-six leagues from the seashore, and forty-two leagues from any other volcano. This phenomenon remained unknown to the mineralogists and natural philosophers of Europe, though it took place

Large hill thrown up by a volcano in 1759.

* Extracted from his Essay on New Spain. Journal de Physique, vol. LXIX, p. 149.

but fifty years ago, and within six days journey of the capital of Mexico.

Country described.

Descending from the central flat toward the coasts of the Pacific ocean, a vast plain extends from the hills of Aguasarco to the villages of Toipa, and Patatlan, equally celebrated for their fine cotton plantations. Between the picachos del Mortero and the cerras de las Cuevas and de Cuiche, this plain is only from 750 to 800 met. [820 to 880 yards] above the level of the sea. Basaltic hills rise in the midst of a country, in which porphyry with base of greenstone predominates. Their summits are crowned with oaks always in verdure, and the foliage of laurels and olives intermingled with dwarf fan palms. This beautiful vegetation forms a singular contrast with the arid plain, which has been laid waste by volcanic fire.

A fertile plain

To the middle of the eighteenth century fields of sugarcanes and indigo extended between two rivulets, called Cuitimba and San Pedro. They were skirted by basaltic mountains, the structure of which seems to indicate, that all the country, in remote periods, has several times experienced the violent action of volcanoes. These fields, irrigated by art, belonged to the estate of San Pedro de Jorullo (Xorullo, or Juvriso), one of the largest and most valuable in the country. In the month of June, 1759, fearful rumbling noises were accompanied with frequent shocks of an earthquake, which succeeded each other at intervals for fifty or sixty days, and threw the inhabitants of the estate into the greatest consternation. From the beginning of the month of September, every thing seemed perfectly quiet, when in the night of the 28th of that month a terrible subterranean noise was heard anew. The frightened Indians fled to the mountains of Aguasarco. A space of three or four square miles, known by the name of Malpays, rose in the shape of a bladder. The boundaries of this rising are still distinguishable in the ruptured strata. The Malpays towards the edge is only 12 met. [13 yards] above the former level of the plain, called las playas de Jorullo; but the convexity of the ground increases progressively toward the centre, till it reaches the height of 160 met. [175 yards].

shaken by an earthquake,

and a hill raised on it.

They

They who witnessed this grand catastrophe from the top of Aguasarco assert, that they saw flames issue out of the ground for the space of more than half a league square; that fragments of red hot rocks were thrown to a prodigious height; and that through a thick cloud of ashes, illumined by the volcanic fire, and resembling a stormy sea, the softened crust of the earth was seen to swell up. The rivers of Cuitimba and San Pedro then precipitated themselves into the burning crevices. The decomposition of the water contributed to reanimate the flames, which were perceptible at the city of Pascuoro, though standing on a very wide plain 1400 met. [1530 yards] above the level of the playas de Jorullo. Eruptions of mud, particularly of the strata of clay including decomposed nodules of basaltes with concentric layers, seem to prove, that subterranean waters had no small part in this extraordinary revolution. Thousands of small cones, only two or three yards high, which the Indians call ovens, issued from the raised dome of the Malpays. Though the heat of these volcanic ovens has diminished greatly within these fifteen years, according to the testimony of the Indians, I found the thermometer rise to 95° [if centig. 203° F.] in the crevices that emitted an aqueous vapour. Each little cone is a chimney, from which a thick smoke rises to the height of ten or fifteen met. [11 or 16 yards]. In several a subterranean noise is heard like that of some fluid boiling at no great depth. The event described

Amid these ovens, in a fissure, the direction of which is from N. N. E. to S. S. E., six large hummocks rise 400 or 500 met. [440 or 550 yards] above the old level of the plain. This is the phenomenon of Monte Novo at Naples repeated several times in a row of volcanic hills. The loftiest of these huge hummocks, which reminded me of the country of Auvergne, is the large volcano of Jorullo. It is constantly burning, and has thrown out on the north side an immense quantity of scorified and basaltic lava, including fragments of primitive rocks. These grand eruptions of the central volcano continued till February, 1760. In the succeeding years they became gradually less frequent. The Indians, alarmed by the horrible noise of the new volcano, at first deserted the villages for seven or eight leagues Six large hills in one line.

round the plain of Jorullo. In a few months they became familiar with the alarming sight, returned to their huts, and went down to the mountains of Aguasarco and Santa Ines, to admire the sheaves of fire thrown out by an infinite number of large and small volcanic openings. The ashes then covered the houses of Queretoro, more than 48 leagues [120 miles] in a right line from the place of the explosion. Though the subterranean fire appears to be in no great activity* at present, and the Malpays and the great volcano begin to be covered with vegetables, we found the air so heated by the little ovens, that in the shade, and at a considerable distance from the ground, the thermometer rose to 43° [109·4° F.]. This fact evinces, that there is no exaggeration in the report of some of the old Indians, who say, that the plains of Jorullo were uninhabitable for several years, and even to a considerable distance from the ground raised up, on account of the excessive heat.

Ashes carried to a great distance.

The air still heated by the subterranean fire.

Near the cerro of Santa Ines the traveller is still shown the rivers of Cuitimba and San Pedro, the limpid waters of which formerly refreshed the sugarcanes on the estate of Don Andrew Pimantel. These springs were lost in the night of the 29th of September, 1759: but 2000 met. [near 2200 yards] to the westward, in the soil that has been elevated, two rivulets are seen to break out of the clayey dome of the furnaces, exhibiting themselves as thermal waters, in which the thermometer rises to 52·7° [126·86° F.]. The Indians still give these the names of San Pedro and Cuitimba, because in several parts of the Malpays large bodies of water are supposed to be heard running from east to west, from the mountains of Santa Ines to the estate of the Presentation. Near this estate is a brook, that emits sulphuretted hidrogen gas: it is more than 7 met. [near 8 yards] wide, and is the most copious hidrosulphurous spring I ever saw.

Two rivers lost,

and supposed to appear again much heated.

Sulphurous water.

* In the bottom of the crater we found the heat of the air 47° [116·6° F.], and in some places 58° and 60° [136 4° and 140°]. We had to pass over cracks exhaling sulphurous vapours, in which the thermometer rose to 85° [185°]. From these cracks, and the heaps of scoriæ that cover considerable hollows, the descent into the crater is not without danger.

In

In the opinion of the natives these extraordinary changes I have described, the crust of earth raised and cracked by volcanic fire, the mountains of scoriæ and ashes heaped up, are the works of monks; the greatest, no doubt, they ever produced in either hemisphere. Our Indian host, at the hut we inhabited in the plain of Jorullo, told us, that some missionary capuchins preached at the estate of San Pedro, and, not meeting a favourable reception, uttered the most horrible and complicated imprecations against this plain, then so beautiful and fertile. They prophesied, that the estate should first be swallowed up by flames issuing out of the bowels of the Earth; and that the air should afterward be cooled to such a degree, that the neighbouring mountains should remain for ever covered with ice and snow. The first of these maledictions having been so fatally verified, the common people foresee in the gradual cooling of the volcano the presage of a perpetual winter. I have thought it right to mention this vulgar tradition, worthy a place in the epic poem of the jesuit Landivar, because it exhibits a striking feature of the manners and prejudices of these remote countries. It shows the active industry of a class of men, who, too frequently abusing the credulity of the people, and pretending to possess the power of suspending the immutable laws of nature, know how to avail themselves of every event for establishing their empire by the fear of physical evil.

The catastrophe supposed owing to the vengeance of some monks.

The situation of the new volcano of Jorullo leads to a very curious geological observation. It has already been observed in the 3d chapter, that there is in New Spain a line of great heights, or a narrow zone included between the latitudes of 18° 59′ and 19° 12′, in which are all the summits of Anahuac that rise above the region of perpetual snow. These summits are either volcanoes still actually burning; or mountains, the form of which, as well as the nature of their rocks, renders it extremely probable, that they formerly contained subterranean fire. Setting out from the coast of the Gulf of Mexico, and proceeding westward, we find the peak of Oribaza, the two volcanoes of la Puebla, the Nevado de Toluca, the peak of Tancitaro, and the volcano of Colima. These great heights, instead of

Line of volcanoes in Mexico crossing the chain of hills.

of forming the ridge of the cordillera of Anahuac, and following its direction, which is from S. E. to N. W., are on the contrary in a line perpendicular to the axis of the great chain of mountains. It is certainly worthy remark, that in the year 1759 the new volcano of Jorullo was formed in the continuation of this line, and on the same parallel as the ancient Mexican volcanoes.

Indicate a long interior fissure in the Earth.

A view of my plan of the environs of Jorullo will show, that the six large hummocks have risen out of the earth on a vein, that crosses the plain from the cerro of las Cuevas to the pichaco del Montero. The new mouths of Vesuvius too are found ranged along a fissure. Do not these analogies give us reason to suppose, that there exists in this part of Mexico, at a great depth within the Earth, a fissure stretching from east to west through a space of 137 leagues [343 miles], and through which the volcanic fire has made its way at different times, bursting the outer crust of porphyritic rocks, from the coasts of the Gulf of Mexico to the South Sea? Is this fissure prolonged to that little groupe of islands, called by Colluet the Archipelago of Regigedo, and round which, in the same parallel with the Mexican volcanoes, pumice stone has been seen floating? Naturalists who distinguish the facts offered by descriptive mineralogy from theoretical reveries concerning the primitive state of our planet will pardon me for having consigned these observations to the general Map of New Spain, contained in the Mexican Atlas.

II.

Chemical Inquiry into the Composition of some Weapons and Utensils of ancient Bronze: by Mr. KLAPROTH *.

Copper preferred to iron by the ancients.

WE know from ancient authors, as well as from weapons and utensils dug up in modern times, that men in the earliest ages, and even those that succeeded them, employed

* Magazin Encycloped. Juin, 1808, p. 298. From Gehlen's Chemical Journal.

copper

copper in preference for the fabrication of metallic utensils and weapons. Thus what Herodotus says of the Massagetæ, who used no iron, and whose weapons and utensils were of copper, is more or less applicable to all the nations of antiquity.

They were sooner acquainted with it, as more easily wrought.

The great difference in the exterior characters of the two metals in their crude state leaves no doubt, that men were sooner acquainted with copper, and the method of adapting it to their purposes, than iron. It is probable, that they found copper in large masses, and nearly prepared by nature, as we still meet with it in countries, the mineralogical wealth of which has been little explored. Accordingly in treating the ore by fire they could not fail to observe all the advantages of this metal, both with respect to the richness of its produce, and the facility with which it might be forged. Iron, on the contrary, was not so obvious to men's eyes; and the distinguishing of its various ores, with the art of working them, and forming weapons and instruments of them, could only be the fruit of long experience.

Copper employed in preference to iron, when the latter was well known.

I shall not avail myself of the numerous testimonies of ancient authors to prove, that copper has been employed in preference to iron, as it is sufficient, to appeal to Homer. All weapons, both offensive and defensive, as swords, spear-heads, helmets, and shields, as well as various domestic utensils, were of copper (*χαλχος*); though in Homer's time iron (*σιδηρος*) was used, but less frequently, and hardened by plunging redhot into water. Even when the advantages of iron, and the modes of fabricating it, were well known, men used copper for their weapons; for instance, in the last ages of the republics of Greece and Rome.

The copper was hardened by a mixture of tin.

We know, that copper is not fit for the purposes, for which the ancients employed it. When cast it is porous and brittle; and, when forged, too soft. The ancient weapons and utensils being of a hardness, which this metal does not possess, it was long supposed, that the ancients had some method of hardening copper, as we do iron and steel. But chemical analysis has shown the falsity of this opinion; and demonstrated, that these weapons and instruments were not pure copper, but an alloy of this metal with tin, which we call bronze, and which was the *æs*, brass, of the Romans.

The

This was done in the oldest times.

The weapons, instruments, and statues, which have been dug out of the ground, evidently prove, that the property of tin to impart hardness and density to the metal alloyed with it was known and employed by the most ancient nations. All these objects occur of bronze, but none of pure copper. It is astonishing, that this practice of imparting to copper, by alloying it with a certain portion of tin, a hardness sufficient for sword-blades and other cutting instruments, should have been so generally followed by the ancients, notwithstanding the want of tin mines. All the tin they used they were obliged to procure from the Cassiterides, the present Cornwall, and the trade was exclusively in the hands of the Pheuicians.

The tin fetched from Cornwall:

Various antiquities analysed.

Having had an opportunity of assaying several fragments of metallic antiquities, I conceive it may be of some utility to make public the results, as a supplement to the few accurate analyses hitherto made.

Mode of analysis.

The fragments to be analysed, being first weighed, were put into a phial, into which were poured six or eight parts of nitric acid of the specific gravity of 1·22, and digested in a sandheat, till completely dissolved. The contents of the phial were then diluted with a sufficient quantity of water, and the mixture left to stand, till all the oxide of tin had fallen down, and the azure liquid appeared quite clear. This being poured off, the oxide of tin was collected, washed repeatedly with water, dried, heated redhot, and weighed. It was found, that 100 parts of calcined oxide of tin equalled 80 parts of tin in the metallic state. The nitric solution was tested in the usual way for silver, iron, lead, and zinc. When it was found free from these metals, as in all the following inquiries it proved, it was easy to calculate, by deducting the quantity of tin found, the proportion of copper, which was likewise obtained by the common methods.

1. *Analysis of an antique sword.*

An antique sword described.

In a collection of antiquities at Berlin, found on digging into some ancient graves in the march of Brandenburg, among several articles of bronze, as spear-heads, knives, ornaments, &c., are two swords; but the place where they were

were found is not known. One of these swords was broken, the other entire. Their composition is the same. They are both covered with the green shining rust called patina. The sword in question weighs 17 ounces, and is 20 inches long; the blade 16½, and the hilt, which is rivetted, 3½*. The blade is twoedged, and 1¼ inch broad for two thirds of its length, the other third sloping off to a round point. In the middle it is 3½ lines thick, and slopes to form an edge on each side. Below the hilt, and on each side at the edge, is a part cut out 9 or 10 lines long, and 1 or 1¼ deep, the use of which I do not know.

To find the colour and brightness of the blade, I ground it; and though the parts injured by the rust prevented me from restoring its original appearance, its colour and lustre were observable in some parts, and indicated considerable hardness and density.

Its analysis in the way above mentioned gave the following proportions: copper 89, tin 11. Its composition.

To render the description and analysis of these antique swords found in our country more interesting by a comparison with other weapons, I shall here give an abstract of two excellent papers by Mr. Mongez in the 5th vol. of the Memoirs of the Institute, which contain a description and analysis of similar bronze swords found near Abbeville. One was found under a bed of peat, with the skeletons of a man and horse. Its whole length was 22 inches, the breadth of the blade 16 lines, the weight 21 ounces. According to the analysis of Mr. Darcet, it contained 15·53 tin, and 87·57 copper †. A second, which was found at the depth of 10 feet, in a calcareous tufa, was about 29 inches long, and contained fifteen tin and 85 copper. The rivets that fastened the outer part of the hilt contained but 5 per cent of tin, because they required to be more flexible. A third was 33 inches long, and found at the depth of nine feet in a bed of peat, by the side of the skeleton of a man, on the head of which was a bronze helmet. This skeleton and Compared with three others found in France.

* As I do not know, whether the measures in this paper be French or German, they are left as in the French Journal. C.

† One of these two numbers is evidently wrong, but I know not which. C.

several

several others were lying in an ancient boat. The composition of this sword was 10 tin and 90 copper. Another sword, or cutlas, 18½ inches long, contained but 4 per cent of tin.

The bronze instruments of the ancients were cast not forged.

These antique swords were not forged, like our weapons of iron and steel, but were cast in moulds, like all other instruments of bronze. Their edge, as well as those of cutting instruments in general, must have been given by hard, smooth stones.* The opinion of some antiquaries therefore, who assert, that the ancients were unacquainted with the art of casting metals, is absolutely false.

To say nothing of the nature of bronze rendering it incapable of being prepared in any other way, any one may be convinced of this by simple inspection; and if you would have a proof of it in Homer, you need only read the 23d book of the Iliad.

2. *Analysis of the metallic alloy of crooked antique knives.*

Crooked antique knives.

In several provinces of Germany cutting instruments, shaped like sickles, have been found in digging or ploughing the ground; but whether they really were ancient sickles is not determined, as many suppose, that they may have been used as knives in the warm baths. I have selected two of these, found at different places, for analysis.

One, which was found with various utensils in a garden at Merz, near Mueltord, yielded by analysis, after its crust of grayish rust was removed, tin 15 parts, copper 85.

The other, found in the island of Rugen, was covered with the common patina, and gave tin 13, copper 87.

3. *Analysis of an antique ring.*

An antique ring, of elastic and flexible bronze.

I had selected for other inquiries a fragment of an elastic and flexible ring, which was found with some Roman coins in the vicinity of the Rhine. This ring was made with a half-flattened stem, grooved on the outside, and 8 lines broad. Its exterior diameter is 2¾ inches, its interior 2¼. It is not soldered, but its extremities are so closed by the elasticity of the metal, that it is difficult to separate them. The colour of the metal, in the parts that have

been

been polished, is very fine. We have no sufficient clew to the use of these rings. Its analysis gave tin 9, copper 91.

The same proportions were found in an elastic ring analysed by Mr. Mongez, which was found near Bourg, where several other Roman antiquities had before been discovered.

It is to be wished, that the elastic property of bronze should be examined more minutely.

4. *Analysis of a piece of Grecian brass.*

This little fragment, decorated with ornaments, which was found in Sicily in a Grecian tomb, appears to have been a button, or some other ornament of armour. Its proportions are, tin 11, copper 89. Grecian brass.

5. *Analysis of antique rivets.*

These rivets were short, and of the thickness of a middle-sized wire. As it was necessary they should be flexible, it was requisite, that the alloy should be in different proportions, that of the tin being diminished. This consisted of tin 2·25, copper 97·75. Antique rivets.

6. *Analysis of an antique cup.*

The great number of antique cups and vases found at different times sufficiently prove, that the ancients possessed the art of reducing bronze to thin sheets. The cup, pieces of which were employed for this analysis, was found in a Grecian tomb near Naples. It has so well resisted rust, that its inside has lost very little of its polish. Being very thin, I expected to find in it but a small proportion of tin; but I obtained tin 14, copper 86. Antique cup.

Comparing the proportions of tin found in the present analysis with those of a fragment of an antique mirror, which I had already published in Scherer's Journal, vol. VI, and which consisted of 32 per cent tin, and a little lead, we find, that the ancients judiciously adapted the proportions of tin and copper to the purposes, for which they were required. I conceive it unnecessary to particularize the rest of the analyses I made of pieces of antique bronze: Antique mirror.

bronze: it is sufficient to say, that, except this mirror and the rivets already mentioned, I always found the alloy contained from 9 to 15 per cent of tin.

7. *Analysis of the quadriga of Chios.*

Quadriga of Chios.

The proportions of the alloy of this masterpiece of antiquity bear no resemblance to those already mentioned. It has been long asserted, that these horses were the work of Lysippus, contemporary of Alexander, who is known in the history of the arts as the greatest master in the execution of equestrian statues: but several modern connoisseurs dispute this, and say the horses are in too clumsy a style, to have been the work of Lysippus.

History.

It is admitted however, that they were brought from Chios to Constantinople in the reign of Theodosius I. In 1204, when the croisaders made themselves masters of that city for the second time, pillaged it, and set it on fire, this quadriga escaped the destruction, that befel many ancient works of art. On dividing the plunder, the doge Dandolo destined these horses for the republic of Venice. After his death the podestat Martin Zeno sent them to Venice with other parts of the spoil, and the doge Peter Ziani ornamented with them the entrance to the cathedral of St. Mark. About six centuries after, in 1798, they were removed to Paris, and placed at the two entrances of the square of the Carrousel. Since that time they have been brought together again, and harnessed to a chariot, to decorate the triumphal arch in that square.

Fabrication.

These four horses were not cast at once, like statues in bronze, but are composed of separate parts, wrought with the chisel, and afterward joined together. The hollows in the hind parts are filled with lead, which has assumed its shining reddish appearance. These parts are gilt: yet the gilding is nearly effaced, though, according to Buonarotti, the gold with which the ancients covered their bronze was to ours as six to one.

Of copper

These horses were supposed to be of copper, because this metal takes gilding better than bronze; and I have been enabled to verify the fact on a small piece weighing 40 grains, which was sent me. From this it appears, that the copper was

was not absolutely pure, as it contained a little tin; but the oxide of tin obtained from these 40 grs. amounted only to 0·35 of a grain; so that, when reduced to the metallic state, the proportion would be only 7 parts of tin to 993 of copper. This proportion is so small, it may be presumed to have been accidental. with a very little tin.

In our days the use of iron and brass has singularly diminished that of bronze, which was so frequently employed by the ancients. It is now confined to cannons, bells, and statues *. But is it not desirable, that our copper vessels should be replaced by vessels of bronze or brass, as they are less liable to oxidation, and to injure the health? This question deserves to be solved by comparative experiments. What ought to induce us to examine this important question is, that the ancients employed only vessels of bronze in their kitchens and cellars in general, though they were well acquainted with the injurious qualities of oxide of copper taken internally. This oxide however they used externally for cleansing and healing wounds. According to Aristotle, wounds made with weapons of bronze were more easily cured than those made with weapons of iron. Might not bronze be advantageously employed for utensils in our days?

In a note subjoined Mr. Darcet observes, that the metal of the horses of the Carrousel, taken as it is, yields copper, tin, lead, gold, and silver. If the surface be filed, so as to remove all the gilt part, nothing is found but copper, tin, and lead. If a piece perfectly free from cracks be taken, and thoroughly cleaned by the file, it yields copper and tin alone: but it is difficult to procure such pieces, for the copper is full of flaws, and the mixture of lead and tin, with which the horses were partly filled, has insinuated itself into every crack. On analysing some select pieces, he found copper 99·177, tin 0·823: but as sulphuric acid disturbed the transparency of the solution, he supposes a little lead was present, and that part of the tin might come from the alloy of tin and lead, which had covered the inside of the pieces he used. Metal of the quadriga of Chios examined by Darcet.

* Mortars for apothecaries and druggists too are commonly made of it. C.

He

Gilding.

He could not procure a piece well gilt, to examine in what way the gold was applied; but he observes that the brittleness of the metal seems to indicate, that quicksilver was employed.

III.

On the black Sand, or Menachanite, found on the Coasts of Liguria: by D. VIVIANI, *Prof. of Botany and Natural History in the Imperial Academy of Genoa*.*

Situation of minerals to be studied.

THE situation of minerals becomes daily more important to be known, as it furnishes the geologist with knowledge of importance to the theory of the Earth, and the mineralogist with characters highly interesting with respect to that kind of association, which takes place between different minerals.

That of menachanite unknown.

The situation of the ferriferous oxide of titanium has not yet been studied in a satisfactory manner. There is even a variety of this species, known by the name of menachanite, which has hitherto been found only in the form of sand, and the situation of which is consequently unknown to the naturalist. The black sand found on the seashore between Pegli and Sestri, about four miles from Genoa, having been analysed by my learned colleague, prof. J. Mojon, was found to be true mechanite, of which it has all the mineralogical characters. Hitherto it had been found only on the seashore, where it never appeared but after great storms, which had confirmed the opinion of its submarine situation, and destroyed all hope of ascertaining it.

Supposed to be washed up by the sea,

I had visited the shore however several times between Pegli and Sestri, after pretty violent storms at sea unattended with rain, without finding any appearance of menachanite; on the contrary I found a stripe skirting the sea for a hundred paces or more, if the storm at sea had been accompanied with abundant rain, and particularly if the brooks

but appears on the shore only after land floods,

* Journal de Physique, vol. LXIX, p. 314.

brooks of Sestri and Pegli had inundated this part of the shore, after washing the surface of the surrounding mountains.

This observation, frequently occurring, led me to suspect, that the black sand on this shore was furnished by the mountains, that pour their waters into the Varenna, as well as by other torrents running between Sestri and Pegli; that the conflict between these fresh water streams and the waves of the stormy sea directed against the shore effected a washing of the earth containing the mechanite; and that thus the mineral was deposited in the form of sand on the shore, while the earthy substances, being less heavy, remained some time longer diffused and suspended in the seawater.

therefore probably brought from the mountains.

This conjecture inspired me with the hope of finding the native bed of the mechanite in the mountains above Sestri and Pegli, which form the streams of the Varenna and other little torrents, that flow across that shore. I determined therefore to trace these torrents to their sources, and at the same time ascertain the nature of the mountains.

Torrents traced to their sources in Liguria.

The torrent of Varenna divides the territory of Sestri from that of Pegli. Though dry in summer, it is above a hundred yards broad at its mouth. It forms the edges of the plains that border it on both sides, and adds to them annually; but it frequently resumes its rights, and destroys in a day the work of ages.

The torrent comes from a chain of magnesian mountains, which shelter the fine plantations of Pegli from the north wind, and render its climate the mildest in all Liguria. To the right of this torrent, above Pegli, begins a system of mountains of micaceous schist, which runs sometimes into shining at others into micaschist. This system is continued to the south-west all along the Apennines, and forms the nucleus of these mountains in the western range to their junction with the maritime Alps. It is between Pegli and Sestri only that we can properly establish the commencement of this system; for the slaty schist, and secondary limestone, extend from the serpentine chain of Bracco, in the eastern range, as far as the Scoglio de St. Andrea, a distance of about eight and twenty miles, where they suddenly disappear,

Mountains from which they proceed,

disappear, and are replaced by transition rocks, till we come to the micaceous schist*.

I sought in vain for the black sand in the bed of the Varenna. The pebbles with which it is loaded, and still more the fine sand and clay, which in the alluvions are last deposited on the bed of the torrent, do not allow it to be discovered.

I then directed my search to the slope of the mountains, both on the side of Pegli and on that of Sestri; to examine whether mechanite were one of their component parts.

Menachanite found at their feet.

My first discovery of menachanite was in the little wood Grimaldi, above Pegli, on the right of the Varenna, about 1 kil. [3278 feet] above the level of the sea, and about 100 met. [328 feet] from the place where the menachanite had hitherto been found†.

The night before I observed this a very heavy rain had fallen. The streams descending from the mountain had furrowed the ground in all directions, and let fall on the borders of the furrows the gravel they had washed down. On these borders I soon saw the black sand, which had every appearance of the menachanite on the shore of Pegli, and which was found on examination to be perfectly similar to it.

This sand traced to some distance.

I then traced this black sand to the beginning of the edge of the plain between the foot of the mountain and the sea; and no doubt should have been able to trace it to the shore, if the cultivation of this plain, and the successive deposits of the Varenna, which I have already mentioned, had not interrupted my search.

The mountain examined and described.

My next step was to examine with attention the mountain on the back of which I had just discovered the menachanite. It is formed of a micaceous schist, of a silvery gray colour, composed of thin laminæ so tender as to be broken by the nail,

* The different systems of mountains observed in the Apennines of Liguria, and every thing that concerns the natural history of this country, will be displayed in my Tours in the Apennines, which will soon appear.

† Here is evidently some mistake, either in the copy, or of the printer. I imagine it should have been 3278 feet from the shore, and at 328 feet elevation. C.

nail, though they cannot be detached in large scales. These laminæ, in pieces fresh broken, have a silky lustre; and in some places they are as finely striated, as if they were formed of fibres united longitudinally. The latter characteristic and their colour disappear in pieces that have been exposed to the air; an ochrey yellow, a blackish tint, and a gray, mingling together, and destroying the primitive colour.

Particles of mica are seen glistening on some parts of the rock: but, beside these detached scales disseminated through the rock, this mineral exists in it in a state of extreme attenuation, for it produces the silvery lustre, which covers the surface of the stone as with a varnish.

An examination of this schist gave me no indication of mechanite, except the black colour, which sometimes spread through the interior of the rock, and indicated a kind of decomposition; but hitherto my observations furnished me with nothing beyond simple conjecture.

A cut in the mountains showed some parts of the rock in a state of decomposition and containing menachanite,

The formation of an artificial lake above the wood of Grimaldi, having required a perpendicular cut in the mountain, afforded me a favourable opportunity of pushing my inquiries farther. I then observed a kind of stratification in the mountain; but the strata are so disordered, that it is impossible to ascertain their general direction. Some veins of quartz run in the direction of the strata themselves. The black colour, which I had already observed extends into the substance of the schist, mixes with a yellow ochre, which fills the cavities produced by the decomposition of the stone. I examined these cavities with a lens in places where the black colour was deepest, and found that this colour was produced by a pulverulent substance, in shining, angular grains, attracted by the magnet, and in short exhibiting all the mineralogical characters of menachanite.

perfectly resembling that found on the shore.

From the pieces of schist, which I had broken off from the mountain, I selected some, that exhibited throughout large spots of this black substance, mixed with the yellowish ochre I have described. These I powdered in a mortar; and from this powder the magnet separated several grains of menachanite perfectly resembling that on the shore of Sestri and Pegli.

Thus

Thus I can no longer doubt, that menachanite, or the ferriferous oxide of titanium, enters into the composition of this micaceous schist, and occasions its black colour; and that it is these metallic grains, which, in consequence of the decomposition of the rock, are washed down by the waters, and thrown upon the shore after separation by washing.

This sand therefore a component part of a primitive rock.

From these observations we find, that the granular and sandy form, under which menachanite appears, is by no means occasioned, as has hitherto been supposed, by the action of the water washing it along. We have seen, that the menachanite, as soon as it is separated from its gangue, appears in the same granular and sandy form as on the shore of Sestri. The situation of this mineral therefore must be considered as one of the most ancient among metallic substances, since it is found among the component parts of a primitive rock; and appears not to have been in its original state even when this rock was formed. Some analogy may be perceived between these sandy grains of menachanite, and the gold dust, that was erroneously supposed to be separated from ores of this metal, and mixed with sand by the waters. But this gold dust has already been found in soil brought from some other place, which leaves us uncertain as to the period, when the gold was reduced to this form. I see nothing to connect this singular situation of the menachanite with any geological epoch but the sandy depositions, and thin strata of sand, which the illustrious de Saussure has found interposed and almost mingled with the composition of primitive mountains. Menachanite, as well as these sands, may have been reduced to granular fragments in some more remote ages of our globe.

Analogous to gold dust.

Other instances of sand among primitive rocks.

Whatever may be thought of these conjectures, the situation of menachanite in the shining schist of the mountains of Pegli is established. Thus, since the torch of the natural sciences has begun to illumine the land of Liguria, the vicinity of Genoa has already made known the natural situation of two minerals, the variolite in mount Ramazzo*, and the menachanite, which forms the subject of this paper.

Variolite in mount Ramazzo.

* Annales du Muséum.

IV.

IV.

Remarks on some properties of Nickel; by Professor TOURTE, *of Berlin**.

THE preparation of a needle of nickel for the royal mineralogical cabinet at Berlin afforded me an opportunity of making a series of experiments on metallic nickel, which will not perhaps be uninteresting. The piece of nickel employed was fifteen inches long, seven lines broad, and a line and half thick. It was dull and rough, with here and there a metallic lustre. Its colour was similar to that of silver of twelve pennyweights heated red hot. The metal loses but little of its lustre in the air, and it takes a fine polish. This is best effected by smoothing the surface with a fine file, then rubbing it on a bluestone with water, and finishing the polish with tin putty and oil. Burnishing does not answer so well. When well polished, its colour is a mean between steel and platina. If heated redhot, the colour is changed to a grayish green, resembling antique bronze†. The intensity of colour increases in the oxide every time it is heated, and the nickel loses its lustre. Dilute nitric acid removes the oxide, and leaves the metal with some degree of brightness.

Needle of nickel.

The metal described.

In oxigen gas the metal burns, and throws out sparks; but some care is requisite, to make this experiment succeed. When the metal, placed on redhot charcoal, is exposed to a

Burns in oxigen gas.

* Annales de Chimie, vol. LXXI, p. 163. Translated from Gehlen's Journal, by Mr. Vogel.

† Nickel well polished is more liable to be oxided by the air than iron. But it may be blued over the fire like steel; becoming first of a light yellow, then a deep yellow, afterward a light violet blue, and lastly of a deep violet blue. If the temperature be raised, it becomes a blueish gray. The intensity of the colours is not equal to that in steel; yet I found this property in nickel very striking. The blueing requires a higher temperature for nickel than for iron. TOURTE.

May be blued like steel.

 stream

stream of oxigen gas, it is converted into a grayish green oxide*.

Its specific gravity.

Richter gives the specific gravity of this metal cast at 8.279, and when hammered at 8·666: I do not know at what temperature. At 10° Reaum. [54·5° F.], and 27 in. 8 lines [29·4 in. Eng.] of the barometer, I found the specific gravity of nickel slightly hammered 8·402; and thoroughly hammered, of which the needle was composed, 8·932.

Its properties for working.

The difficlty of fusion of nickel is particularly favourable to its being wrought. It is ductile and tenacious, and may even be drawn into the slenderest wire. Soldering it succeeded with me but imperfectly; and I found the rods made by Richter not well united, so that it was difficult to find a uniform bar for a needle. The oxide that forms at a red heat is probably the cause of the imperfection of the soldering, for the parts overlapping each other may be separated without much resistance. When nickel has been exposed to a white heat, and cooled, it may be bent like lead, without breaking: nor does the oxide form a brittle coat, scaling off like that of iron when bent; it is rather pulverulent. The metal has but little hardness and elacticity; and on this account its tenacity and ductility are the greater. It may be filed, but it wears the file. To work it the files should be first soaked in oil. It is quickly heated by filing.

An excellent conductor of heat.

To appreciate its power of conducting heat, I made an experiment with a wire of nickel 7 inches long, 3 lines thick, and weighing an ounce and half. By its side I placed two similar wires, one of copper and the other of zinc. One end of each was inserted into a bit of wax, the other into an iron ball [illegible] inches in diameter. The wax at the end of the nickel melted first; those on the copper wire, and on the zinc, did not melt till afterward. In the course of my operations

Attempt to burn nickel without success.

* Last winter I made some experiments on the combustion of nickel in oxigen gas, in the presence of Messrs. Baader, Ritter, Fuchs, and Horkel; but notwithstanding the fineness of the wire, and all the care I took, I could not succeed. It merely acquired a white heat. We then placed the wire in contact with a watchspring. The latter burned as usual, but the nickel only appeared inclined to burn. In fact its extremity was rounded to a globule, yet it was extinguished as soon as the spring was consumed. This sufficiently proves the difficult oxidability of the metal. GEHLEN.

operations I had frequently noticed the extraordinary manner in which nickel conducted heat, and this led me to make the above experiment.

The magnetic effects of nickel attracted my attention, and I availed myself of the present opportunity, to examine this remarkable phenomenon. Richter has said, that nickel alloyed with copper retains its magnetic property; and that arsenic, on the contrary, is the real destroyer of magnetism. I have no doubt, that arsenic may weaken the magnetic power of nickel, particularly when it is in considerable quantity: but I satisfied myself by my needle of arseniated nickel, that small portions of arsenic weaken it only slightly. Hence I shall never judge of the purity of nickel by its magnetic action.

Its magnetic property not destroyed by small portions of arsenic,

On the other hand I found, that oxigen diminished the magnetic property of nickel, so that there was a marked difference between surfaces more or less bright. The metal I used was divisible with regard to its magnetic property into two portions: the larger was magnetic plus; the smaller, which was about a fifth of the whole, was minus. Between the two was a point of indifference.

but diminished by oxigen. Its poles very different in extent.

I tried the action of heat on the magnetic power. After heating the nickel redhot, I found its action evidently weakened, but its poles were not changed. Heating it redhot a second time diminished its power still more; and after the sixth heating its magnetic property was completely destroyed. A similar effect took place with a powerful needle. I satisfied myself of the ease with which this metal acquires polarity, by placing a piece within the atmosphere of a magnet of moderate power, which acted at 3 inches distance. By this approximation I obtained the same poles as before, but of less intensity. A continued red heat caused the magnetic property to disappear again. A few blows with a wooden mallet reproduced some slight traces of it; and its action became stronger, after the nickel had been forged on the anvil.

Destroyed by heating, and in part restored by forging.

It does not appear probable to me, that the metal, after being reduced, should acquire polarity without the application of a magnet. The conjectures of Richter too appear to me inconclusive, when he examined the magnetic power of nickel

Its polarity probably acquired.

nickel in grains by bringing near them a magnet, which would necessarily render these grains magnetic. It appears certain, that it is more difficult to deprive nickel of magnetism than iron. It would be interesting to science, to examine whether the variation and dip of a needle of nickel be the same with those of a needle of steel. I found the dip of a needle of steel similar to that of a needle of nickel touched by the magnet. They were both to the north.

Its dip the same as that of a steel needle.

V.

Abstract of a Paper on the Tenacity of ductile Metals, the Changes of Density in Lead by the Operation of Pressure, and the Action of distilled Water on this Metal, read to the Physical and Mathematical Class of the Institute, by GUYTON MORVEAU*.

Tenacity of metals differently stated.

THE author of this paper having observed in modern works of the highest estimation expressions of the tenacity of some metals very different from those he had deduced from his experiments, published in the 25th volume of the Ann. de Chim., deemed it of sufficient importance to institute a new examination, and to add at the same time the observations he has since collected for completing the synoptic table of the distinguishing properties of metals, which he had drawn up for his course of lectures at the imperial polytechnic school. Not that he thinks the maximum of this force of cohesion of metals should enter into the calculations of artists who employ them; for it is well known, that the accidental imperfections, which always more or less facilitate their rupture, oblige them to have recourse to larger dimensions; but it is not the less true, that the known ratios of their tenacity are of use to determine which should be preferred, and afford an important test of their purity, and the quality of the manufactured metal.

With regard to copper, platina, silver, gold, and iron,

* Abridged from the Annales de Chimie, vol. LXXI, p. 189.

the

the author's experiments agreed with those of Dr. Thomson; but with respect to other metals he differs from him.

The examination of the tenacity of zinc appears the more to deserve attention, as it appears to be in contemplation, to substitute it for lead in covering roofs of houses; and as, on account of its great dilatability, it was considered by Smeaton particularly advantageous in the construction of compensations for timepieces.

Zinc deserving attention.

Mr. Morveau has examined it in different states: he tried a bar of forged zinc purified by Mr. Vauquelin, sheet zinc from Limbourg given him by Mr. Descotils, and zinc laminated and drawn into wire by Messrs. Praire and Tournu. The expression of the resistance, which was the mean term of eight experiments, places it immediately after gold, as in the following table.

Examined in different states.

Table of the tenacity of different metals.

	kil.	lbs. avoir.
A wire of 2 millim. [0.787 of a line Eng.] in diameter of iron supported before it broke a weight of	249·659	549·250
Copper	137·399	302·278
Platina	124·690	274·320
Silver	85·062	187·137
Gold	68·216	150·753
Zinc	49·790	109·540
Tin	15·740	34·630
Lead (calculating by the dimensions at the point of rupture)	12·555	27·621
(calculating by the dimensions before it stretched)	5·623	12·371

The author would have included nickel in this table, now admitted into the class of ductile metals, and the tenacity of which Mr. Richter presumes must be very considerable; but the trials to which he subjected it gave only a tenacity of 47·67 kil. [104·875 lbs.] for a wire of the above diameter. It is true the piece he tried, which he had from Mr. Vauquelin, and which he considered as pure, did not exhibit in the process of flatting all the ductility, that Mr. Richter announced; whence he inferred, that we must wait for more decisive experiments, before its place in the table can be absolutely determined.

Nickel.

Two

Two observations, that offered themselves to the author in the course of his inquiry, led him to some researches, that form important sections in his paper.

Density of lead diminished by flatting,

The first is the diminution of the specific gravity of lead by the process of flatting, confirmed by numerous experiments of Muschenbroeck, and which Dr. Thomson has related, at the same time confessing, that the cause still remains unknown. Mr. Morveau was particularly induced to clear up this anomaly, as in a paper in the 2d part of the Memoirs of the Institute for 1806 he had recorded a fact, which appears contradictory to this: four men were unable to ram home a cannon ball surrounded by a ring of lead.

unless it is compressed on all sides.

Having verified this diminution of the density of lead, and determined with precision the circumstances accompanying it, when extended under the hammer, passed between rollers, drawn out as wire, and struck with the engine; he perceived, that this effect took place only in proportion to the facility with which this metal softens, this occasioning the metal to escape from the pressure, even when struck in a collar; as was clearly shown by the quantity of the metal that rose at each stroke. He was resolved however to obtain a more direct proof, by stamping planchets of lead in a very strong collar, in which they were completely confined between two plates of iron. In this way their density was gradually increased from 11·358 to 11·368. Hence it is to be concluded, that lead, when confined in a place from which it cannot issue out as if it were fluid, is susceptible, like other ductile metals, of a degree of compression, which brings its particles nearer together, and increases its specific gravity.

Lead readily dissolved by pure water,

The second phenomenon, to which the author's attention was called, was the very speedy action of water on lead; the distilled water, in which it was suspended from the hydrostatic balance, soon acquiring a milky aspect, and a white flocculent sediment being at length deposited in it.

He satisfied himself by a series of experiments, which he gives with their results, that distilled water acts on lead spontaneously, and without the assistance of agitation: that this action takes place even on lead revived from the muriate: that it occurs with water distilled in glass vessels, a circum-

a circumstance that excludes all galvanic influence: that it ceases, when this water has been deprived of air by boiling, or by exposure to the vacuum of an airpump: that it stops, when the air the water was capable of furnishing is exhausted: that it recommences, when air is restored to the water: that the presence of any neutral salt, as the sulphates, nitrates, muriates, in ever so small a quantity, as for instance 0·002 of sulphate of lime, is sufficient to obstruct this action: and that to this is owing the preservation of lead without alteration in the water of the Seine, in well-water, &c., whether in open or in covered vessels. Hence this metal may be considered as one of the most accurate tests of the purity of water, provided the water contain no salt with excess of acid.

as long as the water contains any air:

but not in water containing any neutral salt.

With regard to the nature of the product of this action, there is a manifest oxidation of the metal, but without any decomposition of the water; different from that of iron, or of zinc, which takes place in common water as well as in distilled water, and even in that which is totally deprived of air. It is not a simple oxide however: its lightness; its flocculent form; its silvery lustre; the crystalline points perceptible on the surface of the sediment; the state of litharge of a golden yellow, which it assumes when heated; the rapidity with which the approach of a hidrosulphuret gives it the appearance of a galena in shining scales; and lastly the drops of water, which the heat of the sun extricates from it after it has been long dried in the open air, with the little effervescence it produces in acids; lead the author to suppose, that this product is of the nature of a hydrate.

The product is probably a hydrate.

VI.

Improved mode of preparing Phosphorus Bottles. In a Letter from a Correspondent.

SHOULD Mr. Nicholson think the following observations worthy a place in his valuable Journal, they perhaps may tend to lessen a difficulty occasionally experienced by individuals in the prosecution of a favourite study.

Improved mode of preparing phosphorus bottles.

It probably may be acceptable to G. O., vol. XXV, p. 189, to be informed of a method of preparing a phosphorus bottle, which is in a considerable degree free from the inconvenience attending those prepared according to the method made use of by him.

Phosphorus, cut into small pieces and mixed with quick lime in powder, answers the purpose very well. The phosphorus should be carefully dried by filtering paper, a thin slice being cut may be divided into as many pieces as can expeditiously be done, and each piece introduced into a small bottle, with as much lime, as will surround it. Lime slacked in the air, and submitted to a strong red heat, in a black lead crucible for twenty minutes, is in a good state for the purpose.

The bottle, when full, may be exposed corked, to the radiant heat of a fire, till some of the pieces of phosphorus have assumed an orange tint, it will then be ready for immediate use. But the heating is not absolutely necessary if the bottle is not wanted for immediate use, and it will continue longer in a serviceable state.

It is almost superfluous to observe, in using the bottle the mouth should be closed with the finger as soon as the match is withdrawn.

Bottles thus prepared may be used frequently during four or five months.

I have been in the habit of preparing a bottle by this method, at the conclusion of winter, for the purpose of lighting a lamp furnace during the summer months, when I had not convenient access to a fire. A narrow quarter ounce bottle has generally continued serviceable four or five months, though very frequently used.

Lancaster, 17*th March*, 1810. P.

VII.

Remarks on Professor Leslie's Doctrine of Radiant Heat; with Experiments to show, that Caloric can pass through transparent Media without heating them. By J. D. MAYCOCK, *Esq.*

(*Concluded from page 78.*)

I Proceed, Sir, to offer for your consideration a few observations on Mr. Leslie's experiments, and on my own.

If,

If, as Mr. Leslie affirms, caloric never passes through a medium without first raising its temperature, blackened glass, used as a skreen, should affect the thermometer in a greater degree than plain glass: for by blackening the surfaces of the glass both their absorbing and radiating powers are increased: but we find by Exp. 2d and 4th, that the thermometric rise with plain glass is more than twelve times higher than with blackened glass. Exp. 5th and 8th, however, afford yet more satisfactory evidence of the calorific rays passing through diaphanous bodies independently of heating their substance. Another argument in opposition to Mr. Leslie's opinions, if more were requisite, might be drawn from the results given in Exp. 9th. If his position were well founded, since the effect with plain glass is to the effect with painted glass as 7 to 5; and since, when the hot flask is removed, the painted glass produces an effect as 15; the plain glass should produce an effect as 21. But when the primary source of heat is removed, the effect of the plain glass is to the effect of the painted glass only as 13 to 15.

Caloric passes through transparent media without raising their temperature:

It is an indubitable fact, that, when a transparent sheet of glass is brought near to a hot body, it will have its temperature augmented; but the fairest inductions from my experiments warrant us in concluding, that the use of the thermometric fluid is not solely, or principally, referrible to this secondary source of heat.

Mr. Leslie's position, however, holds perfectly true with respect to the action of opaque skreens. It is rendered probable by every experiment, in which they were employed; and is satisfactorily proved by Exp. 6th, 7th, and 9th. This leads me to notice the results of Exp. 2nd and 3rd. When an opaque skreen is interposed between the thermometer and the hot body, one surface of the skreen absorbs caloric from the hot body, the other surface radiates to the thermometer. The most considerable effect, therefore, should be produced on the thermometer when a skreen is used, the surfaces of which are best adapted for absorbing and for radiating, and the substance of which is the best conductor of caloric. In the two former qualifications the painted tin and the painted glass are on a par; in the latter

but not through opaque media.

ter the painted tin has the decided advantage: yet in Exp. 2nd the thermometer indicated a higher temperature than in Exp. 3rd. I consider this unexpected result as depending on the great difficulty of so completely painting the surfaces of the glass and of the tin as entirely to destroy the transparency of the one, and the polish of the other.

In some instances heat passes through opaque as readily as through transparent media.

We learn from Exp. 12th, that, when a single mirror is employed, and boiling water is the source of heat, caloric does not pass more readily through glass than through an opaque body, and Mr. Leslie's experiments show, that, as the skreen is removed from the hot body, the effect on the thermometer diminishes, and at last entirely disappears. But even these results admit of explanation on principles very different from those assumed by Mr. Leslie.

Attempt to explain this.

We well know, that diaphanous media always intercept a number of the calorific rays; and may therefore fairly conclude, that such media offer a resistance to the passage of caloric. It seems to me reasonable to suppose, that it is by their momentum, that the rays of caloric overcome this resistance, whence I infer, that a perpendicular direction is that most favourable for a ray of caloric to impinge on the surface of glass that it may pass through its substance; and conclude, that the greater is the angle of incidence of a ray, the less will it be able to overcome the resistance offered to its passage. When a single mirror is employed comparatively few calorific rays impinge on the surface of the glass in such a direction as enables them to overcome the resistance; too few sensibly to affect the thermometer. Of the rays not transmitted some will be reflected, some will be absorbed by the skreen, its temperature will be raised, and it will consequently radiate caloric to the thermometer. When therefore we operate with one mirror, the thermometer seems to derive its temperature immediately from the interposed skreen, whether it be plain or painted glass. But when we employ two mirrors, a number of rays are made to fall perpendicularly on the surface of the skreen, they are transmitted by the plain glass; they are intercepted by the painted glass. Hence, in Experiment 9th, with plain glass the thermometer rose as 7, with painted glass only as 5. Yet

Yet we know, that the painted glass is more heated than the plain, for the hot flask being removed, the painted glass skreen occasioned a farther rise as 15 to plain glass as 13.

The best theory of the radiation of caloric supposes it to arise from the repulsive force, which prevails between the particles of this fluid, from which property it is projected in right lines from every body in which it is accumulated. Now as diaphanous bodies admit some rays of caloric to pass through their substance, and intercept others, it seems a probable conjecture, that all the rays of caloric have not the same degree of velocity; and that, the higher the temperature of a body is, the greater number of rays will it project with such a velocity, as fits them for passing through a dense medium. We might therefore with much reason suppose, that a certain number of calorific rays projected from a burning candle, and falling perpendicularly, or nearly so, on the surface of a sheet of glass, would penetrate its substance: that from the boiling sulphuric acid fewer would have the requisite degree of velocity; and from the boiling water still fewer. On such an hypothesis we can readily explain, why with a burning candle the thermometer is brought to a higher temperature than the plain glass screen; why with a flask of boiling sulphuric acid and water the thermometer and screen are at nearly the same temperature; and why with a flask of boiling water the screen is hotter than the thermometer.

Theory of radiant heat.

The higher the temperature the fewer rays will be detained in a diaphanous medium.

Mr. Leslie, having convinced himself, that the calorific ray does not pass through a sheet of ice, considers this fact as an additional evidence of the truth of his position.—But it surely cannot be admitted as such. Experiment proves, that water in its fluid state stops a considerable number of the calorific rays emanated from the sun, which we have reason to believe travel with greater velocity than those projected from bodies artificially heated. Besides, when water freezes, its particles take on a crystallized arrangement; the solid mass becomes porous, whence it is less adapted for the transmission of the calorific ray, and will no doubt be, from the same cause, less pervious to radiant caloric. But the experiment was made with a single reflector, and it has been

Mr. Leslie's experiment with ice not conclusive.

already granted, that with this apparatus the thermometer is immediately affected by the screen, which however affords no support to Mr. Leslie's general position.

His experimentum crucis examined.

As Mr. Leslie considers his 10th as an experimentum crucis, which establishes his theory beyond the power of contradiction; I would occupy a moment of your time in observing, that although we admit, that, when a single reflector is used, the thermometer derives its temperature immediately from the skreen, and that the compound skreen is heated much sooner when the glass surface is opposed to the hot body than when the metallic surface is; yet we by no means find ourselves constrained to adopt Mr. Leslie's hypothesis for explaining the radiation of caloric; as it rests not on these facts, but on the general conclusion deduced from them—That caloric passes through no medium independently of raising its temperature.—A conclusion, which does not necessarily follow from any of the facts or reasonings to be found in the experimental inquiry, and which, from my own observations, I feel myself authorized to reject.

Made with two reflectors it might have turned out differently.

I am inclined to think, that, had Mr. Leslie's experimentum crucis been made with two reflectors, and had he noted the time occupied by it, he would have obtained results similar to those which he has related p. 35 of his book; and they would have admitted of easy and satisfactory explanation on the established principles, which Mr. Leslie considers as wholly unfounded, but to which, on account of their simplicity and apparent truth, I confess myself attached.

Heat reflected only from the surface on which it impinges.

I pointed out in experiment 10th and 11th, that the thermometer is differently affected as the blackened or the plain surface of a sheet of glass is opposed to the hot body. Yet in both experiments the matter of the skreen is the same; and the radiating and absorbing powers are nearly equal. I think the phenomenon may be explained in the following manner. Mr. Leslie's 4th experiment shows, that the calorific rays are reflected only from the surface on which they impinge; the degree of reflection from a glass mirror being always the same, whether the back of the mirror was silvered, preserved clean, or ground with sand or emery. It seems

seems therefore a fair conclusion, that the calorific ray, (projected from bodies artificially heated) after passing through glass cannot be made to return again by any substance placed on the opposite surface of the glass. In experiment 10th the black surface absorbs as much caloric as from its nature it is qualified to do: the skreen has its temperature raised a certain number of degrees, and becomes a source of heat to the thermometer. But in experiment 11th some of the calorific rays are immediately absorbed by the glass surface: others pass through the substance of the glass, but are intercepted by the painted surface: and not being returned through the substance of the glass, they must assist in raising the temperature of the whole skreen, which will consequently become hotter in this than in the former experiment.

I trust, Sir, the experiments I have detailed will be sufficient to satisfy you, that radiant caloric, under favourable circumstances, penetrates glass, and perhaps other diaphanous media, independently of raising their temperature. I shall be glad to hear of their being repeated on a more extensive and varied plan; and regret the want of leisure, which for the present prevents my farther prosecuting this interesting subject. Radiant caloric penetrates some media without heating them.

I have the honour to be, Sir,

Your most obedient servant,

No. 99, Nicolson Street, Edinburgh, J. D. MAYCOCK.
April the 5th, 1810.

VIII.

On the Acrid Principle of Horseradish: by Mr. EINHOF*.

TWO pounds of the root of horseradish, dug up in the month of October, were rasped, and the pulp distilled on Horseradish root distilled.

* Annales de Chim. vol. LXX, p. 185. Translated from Gehlen's Journal.

a water-

a [illegible] and [illegible] and [illegible] of the retort, were [illegible] off. [illegible] ounces of fluid having gone over, the apparatus was [illegible], and the fluid put into a phial.

Product. This product had a very pungent smell of horseradish, so as to occasion considerable pain in the nose of a person smelling to it. It was turbid.

Essential oil. After standing some time, about the quantity of ten drops of an essential oil, of a pale yellow, and of the consistence of oil of cinnamon, was found at the bottom of the phial. Its smell was intolerable, but perfectly similar to that of horseradish root fresh scraped. Its taste was at first sweetish, like that of oil of cinnamon; but it left a burning acrid sensation behind, and the parts of the tongue and lips touched with it became very red and inflamed. A drop of this oil on a pane of glass is very quickly volatilized at the temperature of 12° R. [59° F.], and fills the room with a strong smell of horseradish. It falls to the bottom of water, but mixes with it on shaking, and forms a milky liquor, like that obtained by distillation. Alcohol dissolves it easily and completely.

Water. The distilled fluid is not altered by any reagent, except the nitrate of silver, and acetate of lead. The first changes it brown, and throws down a black precipitate: the second produces a brownish precipitate, indicating the presence of sulphur, which Gutret and Tingry had likewise found in horseradish.

Contains sulphur. The water saturated with essential oil comports itself in the same manner.

Pungency soon evaporates unless in close vessels. The distilled water, or that saturated with oil, if exposed to the air, soon loses its pungent smell, and retains only that of turnips: but in close vessels it preserves its strength for years. Having left a portion of the distilled water, with a few drops of the oil at the bottom, standing in a phial for a twelvemonth, in a cool place, the oil had disappeared, but some small shining needles of a silvery white were formed. These I collected, but the quantity was so small, I could not make an accurate analysis of them; so that I am not [illegible] whether they were benzoic or camphoric [illegible]. [illegible] crystals when dried had a strong smell of horseradish, and irritated the throat. They dissolved [illegible] and incompletely in alcohol. In a spoon over the [illegible] of a candle they

they melted, and diffused a strong smell of horseradish, succeeded by a smell of peppermint, and lastly of camphor. As the heat increased they were entirely dissipated.

The horseradish remaining in the retort, being mixed with water, yielded a few more drops of oil, and a distilled water, with all the properties mentioned above.

I must observe, that, having subjected to analysis some mould, dug up from a depth of twenty feet, I obtained an ammoniacal water, hidrogen gas, and carbonic acid. This compound gas lost its fetid smell, and after a time smelled like horseradish. The water remaining in the jars in which this gas had been collected threw down a black precipitate with solution of silver, which appears to indicate the presence of sulphur.

Deep buried mould analysed.

Smell of horseradish.

IX.

Analysis of the Galvanic Pile. By J. A. De Luc, *Esq. F.R.S.*

Part I.

In January 1806, I had the honour of presenting to the Royal Society two works, connected with each other by the common object of *meteorology*, published at Paris in 1803 and 1804, under the titles of *Introduction à la Physique terrestre par les Fluides expansibles*, & *Traité Elementaire sur le Fluide electro-galvanique*. In the latter of these works I had proved, by direct experiments, that it was the *electric fluid* itself, which acted in the *galvanic pile*; but that it underwent there a *modification*, which made it produce, with an excessively small quantity, some effects, which else it could not produce but with a very great quantity set in motion by the discharge of the Leyden vial, and even of batteries. This fundamental proposition was established in my work by a particular mode of analysis of the effects of the *pile*, which however other avocations obliged me to publish unfinished, but expressing the hope, that some other expe-

Works on meteorology.

Electricity and galvanism identical.

 rimental

rimental philosopher would take it up and follow it; which has not been the case.

Chemical agencies of electricity.

In 1807, I saw in Part I of the Phil. Transactions a *Bakerian* lecture of Mr. Humph. Davy, *on some chemical agencies of electricity*, which revived my attention to this subject. The very ingenious and interesting experiments which distinguish that paper are well known, and my praises would add nothing to those, which it has deservedly received: but a theory was there introduced, which I considered as involving the *electric phenomena* in the thickest veil, and this was my motive for resuming the above experiments.

Mr. Davy's theory.

Mr. Davy supposes a *positive* and a *negative* energy, as belonging to distinct *substances*, constituting a class of *general causes*; and in p. 39, after having specified some of the *bodies* to which he attributes these different *agencies*, he concludes thus: " *In the present state of our* " *knowledge* it would be useless to attempt to speculate on " the *remote cause* of the *electrical energy*, or the reason " why different bodies, after being brought into contact, " should be found differently *electrified*; its relation to *che-* " *mical effects* is however sufficiently evident: may it not " be *identical* with it, and an *essential property* of matter?"

Occult qualities.

Tutored in Bacon's school, I have found in the long course of my study of natural pheromena the profound wisdom of the following passage, in his immortal work *De Augmentis Scientiarum*, lib. III, cap. V. Speaking there de occultis & specificis proprietatibus, which he considers as belonging to a sort of *magia*, the offspring of false *metaphysics*, he says: " Primum enim intellectum " humanum in soporem conjicit, canendo *proprietates spe-* " *cificas* & *virtutes occultas*, & tanquam cœlitus demissas, & " per traditionum susurros solummodo perdiscendas: unde " homines ad *veras causas* eruendas non amplius excitantur " & evigilant, sed in hujusmodi otiosis & credulis opinioni- " bus acquiescunt; deinde vero innumera commenta, & " qualia quis optaret maxime, instar somniorum, insinu- " ant*."

There

* " First it throws the human understanding into sleep, lulling it with sounds of *specific qualities* and *occult virtues*, as if they came down from

There is not in the whole field of natural philosophy an object, to which the application of this warning of a true philosopher can be of greater importance, than that under consideration. Such *formulæ* as Mr. Davy employs in lieu of *causes* may appear satisfactory within the narrow limits of the experiments which he describes; but all the spontaneous phenomena of our globe, in which the *electric fluid* is concerned, are thus thrown into the back ground, and never noticed. There is evidently, however, no greater agent in these phenomena, than the *electric fluid*, which Mr. Davy never considers as a *substance*; he speaks only of *electricity*, of *electric energies*, which are empty words in themselves, when supposed to imply the idea of *causes*; while all the meteorological phenomena proclaim a *fluid*, the *chemical affinities* of which, already manifested, open the road to the most important inquiries.

Should not be applied to the natural phenomena of electricity, which are evidences of a distinct substance.

This consideration was my principal motive in resuming the analysis above mentioned: I have said, that I had not pursued it so far as I saw it possible, but far enough to oppose the erroneous conclusions, into which Mr. Davy's obscure principles had led him, with respect to the *mode of action* of the *galvanic pile*; and continuing that analysis by an uninterrupted series of experiments, I arrived at the following propositions, as conclusions of the whole.

Analysis of the effect of the pile led to the following general propositions,

1. *Positive* and *negative* in electricity are mere *relations* to a certain *standard*, concerning the distributions of the *electric fluid* among bodies, with which *relations* no *chemical* effects are connected.

2. The immediate effect of the combination of two proper metals in the pile, and of their repetition, is to accumulate a certain quantity of *electric fluid* on one half of the length of the pile, which the other half loses.

3. When the two extremities of the pile are connected together by conducting substances, the above property of the pile produces a *circulation* of the *electric fluid*, passing

from Heaven, and could not be learned but by the whispers of tradition: whence men are no more solicitous in the discovery of *real causes*; they acquiesce in these idle opinions, and hence numberless gratuitous comments, which, like those on dreams, every man may apply as he wishes.

constantly from the side on which it tends to accumulate, to the other side, where it compensates the deficiency tending to recur.

4. This *circulation* of the *fluid* produced by a proper association of metals can exist in the same quantity, with the same number of pairs of metals, without either *chemical* effects in the *circuit*, or the *shock*.

5. For the production of these phenomena it is necessary, that a *liquid*, being introduced between the two *metals*, a *alteration* be produced on them. In this case, the *electric fluid* circulating through the *metals* is *modified*, but its *modification* is different according to the *liquid*: with *pure water*, there are *chemical* effects in the *circuit*, but no *shock* is felt; the latter requires, that the circulation be produced by an *acid*.

deduced from experiments. Such were the conclusions deduced from the experiments related in the paper, which I delivered to the Royal Society the 30th of May, 1808; but as this and a following paper have not been admitted into the Phil. Transactions, I shall resume here the same course of experiments and deductions, but by more direct steps, being enabled to do so by the progress of my researches from that time. For this purpose I shall begin by explaining the reason of the plan which I have followed in this inquiry from its beginning in 1800.

Reason of the plan pursued. If we consider a mounted pile without taking notice of its extremities, we see a repeated association of three constituent parts, two *metals* and wet *cloth* or *paper*; which I shall name *silver*, *zinc*, and *wet cloth*, as they were in my fundamental experiments. By considering this composition of the *pile*, the leading circumstances for me were, first, that its *effects* increase with the *number* of the repetitions of the three constituent parts; secondly, that the whole together forms a *conductive* column. These circumstances concur to point out some cause, which accumulates at the extremities of the pile the opposite effects produced in a certain *ternary association* of the component parts: but which is that *association?* In reflecting on this question I was convinced, that, till it was decided, the *mode of action* of the *pile* would remain entirely unknown.

The

The *pile* may be considered as divided into *ternary groups* under three different aspects.—1. *Zinc* and *silver* and *wet cloth* placed *between* them.—2. *Zinc* and *silver* in *mutual* contact with the *wet cloth* on the side of *zinc*.—3. *Zinc* and *silver*, still in *mutual* contact, but the *wet cloth* on the side of *silver*. On this, two questions arose in my mind; first, to which of these *ternary* groups are owing the *accumulation* of the *electric fluid* on one side of the *pile*, and *increase* of its *deficiency* on the other, which become greater with the *number* of these groups? Second, what is the cause, that so small a quantity of *electric fluid*, set in motion by the pile, produces effects, which require so great a quantity of the same *fluid*, when put in motion by any other means hitherto known?

Arrangement of the pile in ternary groups.

On the first queſtion, supposing the *conducting* faculty of the *pile* to be the cause of the accumulation of the opposite effects produced on the *electric fluid*, I concluded, that small metallic *conductors* placed between the really *efficient groups* would not disturb the effects: but that, if these *conductors* were so placed as to produce separate *ternary* associations different from that to which the effects are attached, the latter would cease. Consequently that by effecting the three different *divisions* of the *pile* by small *conductors*, I could not fail to discover the *efficient groups*; a knowledge which might lead to resolve the second question.

Separation of these to discover which is active.

When I proceeded in this plan, I soon found, that no deep analysis of the operations of the *pile* could have been obtained without these admirable instruments, the *gold-leaf* electroscope of Mr. Bennet, and the *condenser* of Sig. Volta. So minute are the quantities of *electric fluid* necessary to be observed in the course of these experiments.

Necessary instruments.

The necessity of having gold-leaf electroscopes directly connected with each extremity of the *pile*, determined the form of the apparatus which I used for these researches, which is represented in Plate III, fig. 1; it consists of two similar *frames*, which may be used separately for other purposes; but for these experiments, they are fixed on the same board, and they form a *pile* divided into two columns A and B. The *frame* of each *column* is composed of three glass

Apparatus described.

Apparatus described

glass rods, covered with insulating varnish, and the assemblage of *groups* contained in each *frame* rests at the bottom on small insulating pillars. The space within the glass rods (except a small part of it at the bottom, in which a section of plates is represented) is, in the *figure*, indicated by only a dotted line; because it varies in height, in the course of the experiments, according to the number of the *groups* and their arrangement. The order of succession in the associated plates of the different *metals* being inverse in the opposite *columns*, these are connected together at their lower parts by a brass slip *d*, which becomes the *middle point* of the *pile*, and its *extremities* are thus transported to the tops of the *columns*.

The upper part of each frame is constructed in the usual manner; the three glass rods passing through a piece of wood, where they are wedged, in order to give to a screw, which passes through the piece of wood, the power to press on the groups at the different heights which they attain, in different stages of the experiments. With each column is connected a gold-leaf electroscope, movable upon a wooden rod: from its top projects a brass spring, which presses on the top of the column, in order to secure a real contact. When the gold leaves diverge, and it is wanted to know whether the divergence is *positive* or *negative*, the electroscope is lifted up only so far as to separate it from the pile, and is then tried in the usual manner.

With respect to *chemical* effects in the *circuit*, I shall confine myself to those which were first observed in *glass tubes* filled with *water*, at the opposite extremities of which enter wires, the latter serving to connect the water with the extremities of the pile. I made these experiments with different kinds of wire, but in the main course of them I used *brass* wires: thus, while *inflammable air* proceeded from one of them, *calcination* took place on the other: these are the only effects which will be considered in the following experiments. For a purpose which I shall explain, I placed two of these *tubes* in the *circuit*, connected together in their lower parts by a *brass ring*, as may be seen at *c*, fig. 1.

In the description of the experiments, the *chemical* effects

fects will be noticed at the end of each wire in the glass tubes; for which purpose, these ends are indicated by the numbers 1, 2, 3, 4. During these *chemical* effects, the *electric states* of three points in the *circuit* will also be investigated; namely, *a*, the point of connexion between the extremity A of the pile and one of the glass tubes; *c*, the ring which connects the two tubes; and *b*, the point of connexion between the second tube and the extremity B of the pile. Apparatus described.

I proceeded at first in a very complicated manner, described in my work, to the intended *dissections* of the pile, by small *conductors* interposed between different *ternary* arrangements of its three component parts; but in resuming these experiments, I used a more simple method, by forming *tripods* with two pieces of small brass wire, soldered together with soft solder in the shape of a T, as represented at fig. 3, Pl. IV, the three extremities of the wires, turned downwards, form three feet *a*, *a*, *a*, one eighth of an inch high, which thus produce the communication of the *tripods* with the group on which they rest by only three small points; and the wires themselves being a little bent downwards, the group above rests also on the *tripod* by three points *b*, *b*, *b*.

In all the experiments, the same order of succession of the *metals* was preserved in both *columns*, notwithstanding their different dissections by the *tripods*: in A, the column begins at the bottom by a *zinc* plate, thus terminating at the top by a *silver* plate; and in B, a *silver* plate begins the column at the bottom, which thus ends at the top by a *zinc* plate. I mention this circumstance on account of a question concerning the denomination of the extremities of the pile by the *metals*, which will occur in the course of the experiments.

From what has been said above of the three different *ternary* groups that may be composed in the same succession of the three component parts of the pile, fig. 2, which represents the different states of the latter in these experiments, will be easily understood; but it must be recurred to, when each experiment is related. No. 1 represents a portion of the *continuous pile*, which is the same from one end

Apparatus described.

and to the other. No. 2, is a *first dissection* of the pile by the *tripods*, in which the latter divide it into *ternary groups* composed of the two *metals* and the *wet cloth* between them; this also prevails from one end to the other of the pile. No. 3 represents the *second dissection* of the pile, in which the *tripods* divide *ternary groups* composed of the two *metals* in mutual contact, and the *wet cloth* in contact with *zinc* only; but this division not being complete at the extremities of the pile, this No. is represented in two parts; one being the top of col. A, terminated by a *silver* plate alone, and the other, the top of col. B, terminated by a *zinc* plate with *wet cloth*. The case is the same in No. 4, shewing the *third dissection* of the pile, in which the *wet cloth* is in contact with *silver* this representation also consists of two parts, one being the top of col. A, terminated by a *silver* plate with *wet cloth*, and the other the top of col. B, terminated by a *zinc* plate alone. In order to leave place for the letters *z* and *s* near the plates, those of *silver* are represented of the same thickness as the *zinc* plates, though they were not so, as I am going to explain.

In order that the frames of my columns might contain the greatest number possible of *groups* when divided by the *tripods*, I used very thin *silver* plates, these being very little acted upon by the liquid during this kind of experiments, each of which lasted only till the different effects were observed, but it is not the same with *zinc*, the surface of which is soon calcined, and must be often restored to the metallic state, so that, on account of the thickness of the *zinc* plates, each column could contain only 38 groups with the tripods, forming a pile of 76 groups, the plates of which were 1·6 inch diameter; and this remained the number of groups in all the following experiments, as far as relates to this part.

Lastly, as the different *liquids*, with which the pieces of cloth may be imbibed, produce different effects, I determined to follow this first course of experiments with two different liquids, namely, *pure water*, and a strong *solution of marine salt*, in order to compare their effects. Thus I made two *sets* of the same experiments, in both of which

I began

I began by a *continuous pile* of 76 groups, and continued by their three different *dissections* with the *tripods*.

I. Set of Experiments, wherein the pieces of cloth were imbibed with pure water.

Continuous pile.

This pile being mounted, and remaining insulated, with only the electroscopes applied to its extremities, their indications, expressed, as they will always be, by the measure of the divergence of the gold leaves in *decimals* of an inch, were at that moment Experiments with the continuous pile.

A, 0·0——B, 0·4 *negat.*

Exp. 1. I tried the *shock* in the surest manner, which is to hold in each hand, thoroughly wet, pieces of metal, for which I used silver spoons, with a drop of water at their extremity but I did not feel any *shock*. Exp 1.

Exp. 2. With the two *glass tubes* filled with water, and applied to the pile as represented in fig. 1 Exp 2.

1. No *electric sign* remained at the extremities; a proof, as I shall have an opportunity of showing, that the *circulation* of the *electric* fluid was produced through the water of the glass tubes.

2. A small stream of *calx* descended from the wire 1.

3. *Inflammable air* ascended from wire 2.

4. *Calx* descended from wire 3.

5. *Inflammable air* ascended from wire 4.

The next point which I had an intention to examine was, whether there were any possibility to discover the *course* followed by the *electric fluid* in its *circulation*, whether from A to B, or the contrary way. This brought to my mind what happens to a stream of water when it meets with a narrow channel in its course; it rises more or less at the entrance, according to the degree of *retardation* which it undergoes, and is proportionably lower below the issue. I thought therefore, that, if there were any *retardation* in the course of the *electric fluid* passing through the water of the glass tubes, there would be some *accumulation* of the fluid at its *entrance*, and *deficiency* at its *issue*, which might be discovered

discovered by connecting with a condenser the three points *a*, *c*, *b*, in fig. 1. I tried a condenser, the plates of which were separated by a piece of *silk*; but in these very delicate experiments, it produced deception; the *silk* retaining some electrisation from one experiment to the other. I was therefore obliged to use a condenser acting by the mere *distance* of the plates, and I constructed one on a new plan, which answers many purposes. The under, or *receiving* plate is horizontally fixed on an insulating pillar. By means of a brass wire with an insulating handle, I connect successively, each time during 20 seconds, the above three points with the under plate of the condenser, with which is connected a gold-leaf electroscope. During the time only that the upper plate is concentrically situate over the former, it is in connection with the ground; after which, by a proper mechanism, this upper plate is made to retire, by revolving on an insulating axis, and I observe the divergence then produced in the electroscope.

Condenser of electricity on a new plan.

I must still mention another circumstance concerning this class of experiments. When the extremities of the insulated pile are only connected with their electroscopes, the indications of the latter differ according to external circumstances, which I shall not introduce here, saying only, that sometimes there are indications on both sides, *positive* at A and *negative* at B, at various correspondent degrees; sometimes also there is only an indication at A, then *positive*; and at other times, as in the above case, the divergence is only at B, and *negative*. But if A be placed in communication with the ground, the whole electric *difference* between the extremities is expressed at B with the *negative* sign; and on the contrary, if B communicates with the ground, the whole *difference* is expressed at A with the *positive* sign. I must also remark here, that there are two different *standards*, very distinctly and judiciously defined in Sig. Volta's system, to which these *comparative* expressions refer. The two *standards* frequently agree, but often also they differ from a cause which I shall explain on another occasion. When the *divergence* of balls or gold leaves in the electroscopes is considered alone, the *standard* of *plus* and *minus* is only the *actual electric state* of the *ambient air*;

Indications of the electroscope differs according to circumstances.

Two different standards.

air; but if we want to know the *electric state* of insulated bodies, the *standard* becomes the *actual state* of the *ground* in that respect. When therefore a body is in communication with the *ground*, it is neither *plus* nor *minus*, and I shall call it *neutral*; meaning in the *electric state* of the *ground*. Neutral.

In the insulated pile, when its extremities are connected together by the glass tubes, if there be some *retardation* of the *electric fluid* in pervading the *water*, it hardly can be perceived, because this is a middle point between the *positive* and *negative* states; but if one of the extremities be placed in communication with the ground, there may remain a *residuum* of the *current* retarded, discoverable by the condenser. This is the reason of the manner in which I have made the observations at the three points *a*, *c*, *b*: I first observed them during the insulation of the pile; then in placing alternately the extremities A and B in communication with the ground, and this I did by the contact of my finger during the communication with the condenser.

Exp. 3. While the *chemical* processes were going on in the glass tubes, I applied the condenser to the three points *a*, *c*, *b*, in the manner above described, and the results were as follows: Exp. 3.

	Without contact.	*With contact of* B	*With contact of* A.
a	*pos.* 0·2	*pos.* 0·3	0·0
c	0·0	*pos.* 0·2	*neg.* 0·2
b	*neg.* 0·1	0·0	*neg.* 0·4

This experiment clearly points out the *direction* of the course of the *electric fluid*, as I expected would happen, if it underwent some *retardation* in pervading the *water* of the tubes: it certainly *enters* the *water* from the side *a* since a *positive residuum* exists at *a* and *c*, when the *contact* is on B: it *goes out* and returns to the pile by the side *b*, since a *negative residuum* exists at c and *b*, when the *contact* is on A; and this is the reason why the middle point *c* passes through three different states; it is *neutral*, when no acceleration is produced in the natural course of the *fluid* by the contact of either of the extremities; but if the course is accelerated Course of the fluid.

by

by more of the *fluid* coming from the ground to the side B, the *middle point* becomes *positive*; and if the acceleration proceeds from some part of the *fluid* flowing into the ground by the side A, the *middle point* becomes *negative*.

Chemical effects of the pile not connected with positive and negative energies.

This experiment shows also how unfounded is the idea, that the *chemical* effects here taken as an example are connected with *positive* and *negative* energies. During the course of the above observations with the condenser, the two *chemical* effects continued sensibly in the same degree at the extremities of the same *wires*, though these underwent changes in their *electric states*. At wire 1, the *electric state* of which is the same as that of the point *a*, *calx* is produced whether it be *positive* or *neutral*. at wire 4, the *electric state* of which is the same as at point *b*, *inflammable air* is produced whether it be *negative* or *neutral*: but the phenomena are still more decisive at the wires 2 and 3, the *electric states* of which are similar to each other and also to that of the point *c*. Now here we see *inflammable air* at wire 2, and *calx* at wire 3, whether they be *positive*, *negative*, or *neutral*. Whence results finally, that the real connexions of these *chemical* effects with the course of the *electric fluid* are the following: *calx* is produced at the wires by which the fluid *enters* the *water*, and *inflammable air* at those by which it *goes out*.

Exp. in which the pile being rendered alternately negative and positive by connexion with an electric machine, the chemical effects were the same.

The same experiments were already related in my work presented to the Royal Society in January 1806, and repeated in my first paper; but in the former, sect. 544, I related another experiment still more striking, which I had made at Berlin with Prof. Erman, who as well as myself was then much employed in galvanic experiments, and possessed a very powerful electric machine. He had at one time an insulated pile of 200 groups of *zinc* and *silver* the size of crown pieces, newly cleaned, by which the usual effects of the production of *calx* and *inflammable air* in the water of a glass tube were going on very rapidly. We connected this pile with the *rubber* of the machine, the *prime conductor* being in connexion with the ground; which was a most powerful way of producing the *negative* state in the *whole* pile, so that it drew sparks from our hands at a great distance; however we perceived no change in the glass

tube,

tube, the *chemical* effects went on with the same rapidity: and the increase of the quantity of the fluid, by connecting the pile with the *prime conductor* of the machine, which made the pile emit spontaneous flashes, did not sensibly increase these *chemical* effects. Not having then to contend with the idea that *negative* and *positive* states were properties of any substance, I concluded only from the above experiments, that our strongest means of depriving the pile of *electric fluid* left always enough of the latter for the *circulation* produced by the property of the former, so small is the quantity thus set in motion; and that the increase of its absolute quantity did not sensibly increase that which *circulates* in the pile.

First dissection of the pile.

This second kind of *pile* is represented at fig. 2, No. 2: the *tripods*, in this *dissection*, are placed between the two *metals*, thus separating *groups* in which these *metals* have between them *wet cloth*. Exp. 4. Wet cloth in contact with each metal, & the metals separated.

Exp. 4. The experiments made on the *continuous pile* having been detailed, I shall only assemble under this head the comparative effects of the two *piles*.

1. This new mode of experiments having been made immediately after the former, the *free pile* (I mean without connexion either between its extremities or with the ground) had the same *electric* indications.

2. No *shock* was perceived.

3. When the *glass tubes* were applied, the same *chemical* effects were produced in them, at the same *wires*; they only began later, and were a little smaller.

4. The condenser applied to the three points *a*, *c*, *b*, manifested, in the same circumstances, the same *residua* of *electric signs*.

At first it might be thought, that this *dissection* indicates the *efficient groups* for both *electric* and *chemical* effects; but the following experiment will show, that the *efficient groups* for these two functions are different.

Second

Second dissection of the pile.

Exp. 5.
The metals in contact, the wet cloth touching the zinc only.

In this *pile*, represented at fig. 2, No. 3, the *tripods* separate *groups* composed of the two *metals* in *mutual contact*, and the *wet cloth* in contact with only the *zinc* plates.

Exp. 5.—1. The *free pile* had the same *electric indications.*

2. There was no *shock* produced.

3. The *glass tubes* being applied, the *electric indications* ceased absolutely at the extremities of the *pile*, a proof that the *circulation* of the *electric fluid* was produced; however no *chemical* effect appeared in the *water.*

4. The condenser being applied to the points *a*, *c*, *b*, no *retardation* was manifested in the course of the fluid.

This experiment begins to show, that the *causes* of production of *electric* and *chemical* effects, by the *pile*, are different; since here the former continue, but the latter are not produced.

Third dissection of the pile.

Exp. 6.
Metals in contact, the wet cloth touching the zinc only.

This *dissection* is represented at fig. 2, No. 4: the *tripods* here divide the *pile* into *groups* composed of the two *metals* in mutual contact, but the *wet cloth* in contact only with *silver.*

Exp. 6. Cessation of every effect: no *electric* sign at the extremity of the *free pile*; no *chemical* effect in the *glass tubes* when connected with it; no *electric* sign at the points *a*, *c*, *b*.

Before I come to the conclusions, concerning the *mode of action* of the *pile*, which may be derived from this first *set* of *experiments*, in which the pieces of *cloth* were imbibed with only *pure water*, and no *shock* was produced, I shall relate the second *set* of experiments, in which a strong *solution of marine salt* was used; as all the results, except with respect to the *shock*, remaining the same, their union will render the conclusions more certain.

II. *Set*

II. *Set of Experiments: wherein the pieces of Cloth were imbibed with a strong solution of Marine Salt.*

Exp. with strong solution of muriate of soda.

Continuous Pile.

Exp. 7. In order to have a point of comparison of the respective *electric effects* of the two *liquids*, I first mounted the *continuous pile* of 76 *groups* with *pure water* in the *cloth*, and observed the *electroscopes* at its extremities: they were this day A, 0·1 *pos.*; B, 0·3 *negat.* I then imbibed the *cloth* with the *solution of marine salt*, and the electroscopes had the same indications. Exp 7.

Exp. 8. Concerning the *shock*. Exp. 8.

1. In placing my fingers, when *dry*, on the extremities of the pile, I felt no *shock*.

2. My fingers being *wet*, I felt a small *shock*.

3. Taking then the *silver spoons*, as above indicated, I had a *shock* up to the elbow.

4. Whenever I withdrew one of the spoons, on bringing it again into contact, I felt a new *shock*; but when, after having felt a *shock*, I kept both spoons steady on the extremities, all sensation ceased. This is an important fact, to which I shall refer hereafter.

Exp. 9. I applied the two *glass tubes* as in *Exp.* 2. Exp. 9.

1. Every circumstance, with respect to *chemical effects* in the *water*, was the same, except that these effects appeared sooner, and were stronger; and every visible *electric sign* ceased at the extremities of the pile as in the other experiments.

2. I tried the *shock* with the spoons; it was not quite so strong as before the *glass tubes* were placed. I fixed also both tubes on the extremities of the pile; every sensation ceased, but observing then the *chemical effects* in the water of the tubes, I saw, by the quantity of *inflammable air* emitted, that they were reduced to about the half of their intensity: whence it appears, that my body was a *conductor* of the same nature as *water*.

3. Taking off the ring which connected the *glass tubes* by the hooks of their lower wires, and placing the angle of the handle of one spoon in one of these hooks, when I touched

touched with the second spoon the other wire, I felt a strong *shock*: but when I succeeded (which is difficult) in placing the angle of the other spoon steady in the other hook, all sensation ceased, and the *chemical effects* went on in the *tubes*.

Exp 10. *Exp.* 10. Repetition of *Exp.* 3; or of the application of the condenser to the points *a, c, b* in the *circuit*.

	Without contact.	*With contact of* B.	*With contact of* A.
a	*pos.* 0·15	*pos.* the gold leaf struck	0·0
c	0· 0	*pos.* 0·6	*neg.* 0·5
b	*neg.* 0·45	0·0	*neg.* the gold leaf struck.

These results are of the same *nature* as in the experiment with *pure water*, but the *quantities* are much increased, showing a greater *retardation* of the *electric fluid* in its course: a circumstance which will be noticed hereafter.

First Dissection of the Pile.

Exp. 11. *Exp.* 11. I shall assemble here all the points correspondent to those of the above experiments on the *continuous pile*.

1. The *same electric signs* at the extremities of the *free pile*.

2. The same effects concerning the *shock*.

3. The same *chemical effects* in the *glass tubes*, only smaller.

4. The same *residua* of *electric signs* at the points *a, c, b*.

Second Dissection of the Pile.

Exp. 12. *Exp.* 12. Assembling also here the comparative results.

1. The same *electric signs* at the extremities of the *free pile*.

2. No *shock*.

3. No *chemical effect* in the *glass tubes*, though the *electric signs* had ceased.

4. No *residua* of these *signs* at the points *a, c, b*.

Third

Third Dissection of the Pile.

Exp. 13. Cessation of every effect with this *dissection*, as in *Exp.* 6.

Such are the leading experiments with respect to the *mode of action* of the *pile*; but before I come to their general conclusions, I must return to the particular circumstance belonging to *Exp.* 5 and 12, namely, that at the same time that no *chemical effects* were produced in the *water* of the *glass tubes*, no *residua* of *electric signs* remained perceptible in the points *a*, *c*, *b* of the *circuit*. This circumstance might appear favourable to Mr. Davy's idea on the *mode of action* of the *pile*, thus expressed at p. 45 of his paper above mentioned. Exp. 13. Circumstance apparently favourable to Mr. Davy's theory

"In the voltaic pile of zinc, copper, and a solution of "muriate of soda, in what is called its condition of *electric* "*tension*, the communicating plates of *copper* and *zinc* are "in opposite electrical states. And with respect to *elec*-"*tricities* of such very *low intensities*, water is an *insulating* "body: every copper plate consequently produces, by "*induction*, an increase of *positive electricity* upon the "opposite zinc plate; and every *zinc* plate an increase of "*negative electricity* on the opposite copper plate; and the "*intensity* increases with the number, and the *quantity* "with the *extent* of the *series*. When a communication is "made between the two *extremities*, the opposite *electri*-"*cities* tend to *annihilate* each other; and if the *fluid me*-"*dium* could be a substance incapable of *decomposition*, "the *equilibrium*, there is every reason to believe, would "be restored, and the *motion* of *electricity* cease."

I shall not consider for the present that system in itself, but only on account of the connexion which it may appear to have with the above 5 and 12 experiments. For as at the same time that, in these experiments, the process called *decomposition* of *water* had ceased in the *glass tubes*, no *residua* of the *electric fluid* were perceptible by the condenser in any part of the *circuit*; it might be supposed, according to Mr. Davy's idea, that the *motion* of the *electric fluid* had really *ceased*. But the following experiments will show, that the cause of no *residua* appearing in the differently explained.

cases here referred to is, that the *circulation* of the fluid in these cases becomes *too rapid*, to be discovered by that *test*.

Exp. 14.

Exp. 14. After *Exp.* 12, I took off the *glass tubes* from the pile.

1. The electroscopes at its extremities, then free, indicated, A, 0·0, B, *neg.* 0·4.

2. I produced the communication between the two extremities by a *brass* conductor; every *electric sign* ceased, even to the *condenser*.

3. I substituted a *slip* of *deal*, which I had left a long while in *water*, in order that *water* itself might become the *conductor*: it produced the same effect as *brass*, every *electric sign* disappeared.

4. I left this *slip* in the same situation, in order to observe the effects in the progress of *drying*. The *water* had already disappeared at the surface, and no *electric sign* was perceived, even by the *condenser*: after some time, the *condenser* manifested these signs, *positive* at A, and *negative* at B; at last the same signs became visible in the electroscopes of the *pile*.

Electric fluid circulates through the pile.

I think it now evident in general, that an actual *circulation* of the *electric fluid* is produced, through the *pile* and any *conducting* substance which connects its extremities; that this *circulation* is naturally so *rapid* when the *electric fluid* does not undergo any *retardation* in the *conductors*, that its course cannot be manifested even by the condenser; and that when it is manifested by *electric signs*, these are produced by some *retardation*.

Its course may be retarded by the nature of the conductors, or modification of the fluid: the first a test of the insulating property of varnishes.

Now: a *retardation* in the course of the *electric fluid* set in motion by the *pile* may proceed, either from the *nature* of the *conductors*, as in the above experiment; or from some *modification*, which the *electric fluid* itself undergoes in pervading the *pile*. The former cause of *retardation* of the *electric fluid*, manifested by the preceding experiment, affords, as I shall show in another paper, the best and easiest method of trying the *insulating* faculty of the different *varnishes* laid on *glass* for supports of electric apparatusses; an essential object in practical electricity; but the latter, namely, the different *modifications* which the *electric fluid* undergoes

undergoes by pervading different *piles*, will become an important object in the course of the conclusions from the above experiments, to which I now come.

The first of these conclusions will concern the fundamental *mode of action* of the *pile*, in its two different effects, *electric* and *chemical*. When I devised the different *dissections* of the *pile* by small *metallic conductors*, I expected, that it would lead to the discovery of the *efficient groups*; this has happened, but in an unexpected manner; the *efficient groups* are not the same for the *electric* and *chemical* effects. For *electric effects*, the *efficient groups* consist simply of the *binary* associations of the two *metals*; each group being separated from the next by a *conducting substance nonmetallic*. Electric and chemical effects produced by different groups. Electric group;

For *chemical effects*, the *efficient groups* are *ternary*; they are composed of the two *metals*, having *between* them a *liquid* in contact with both, which here is in the *wet cloth* Chemical group.

If *Fig*. 2 is considered with that view, it will confirm these determinations. In the *piles*, No. 1 and 2, which produce both effects, the two kinds of *groups* exist. The *binary groups* of metals, to which are owing the *electric effects*, are formed, in No. 1, by the immediate contact of the two *metals*; and in No. 2, they are produced by the interposition of the brass *tripods*; and in both piles, these *binary groups* are separated by a *nonmetallic* conducting substance, which is the *wet cloth*. The condition of *chemical effects* in the *circuit*, which is the *ternary groups* above defined, exist also in these two piles, as in each the *wet cloth* is placed *between* the two *metals*, in contact with *both*. Both in piles 1 and 2:

But the case is different in the piles No. 3 and 4. In the former indeed, the condition of a *motion* of the *electric fluid* exists, namely, the *binary groups* of *metals*, separated by the *nonmetallic* conducting substance; and the *electric effects* continue: but the condition for *chemical effects* in the *circuit* is wanting; the *wet cloth* is in contact only with *zinc* and not with silver; in a word, it is not *between* the two *metals*, which is the condition. electric only in 3:

Lastly, the pile No. 4, which produces neither *electric effects*, nor *chemical effects* in the *circuit*, is deprived of the conditions of both. The *electric effects* are not produced, neither in 4.

 because

because *zinc* is between two metals which have the same relation with it in this respect; *silver* on one side, and the *brass* of the tripod on the other side. The condition for *chemical effects* in the *circ..* is also wanting; as the *wet cloth* is in contact only with *silver*.

These conclusions with respect to the de ermined difference between the two kinds of *groups*, t which I was first led by the above experiments, will be hereafter confirmed by more precise phenomena, sh wn by new experiments directed to that purpose bu mu rst remove an apparent contradiction between these experiments, and what had beer determined by other experimental philosophers on the *electric states* of *zinc* and *copper* when in mutual contact.

Apparent contradiction; either metal seeming in some cases positive, in others negative;

It may be seen in the construction of the four different *piles* represented in *Fig.* 2, that the succession of *zinc* and *silver* plates continues the same in all, throughout the whole length; from which arrangement, the termination of the extremity A is by a *silver* plate, and that of the extremity B by a *zinc* plate; at the same time it has been seen in all the experiments made with these different *piles*, that the extremity A is the *positive*, and the extremity B the *negative*. This seems to imply, that the *silver* side is the *positive*, and the *zinc* side, the *negative*; whereas by other experiments, *silver* is *negative*, and *zinc positive*, when they are connected together Such opposite conclusions from facts equally certain much embarrassed me, till by the study of what is represented in *Fig.* 2, and the conclusions to which it led me concerning the two different *efficient groups*, the riddle was solved i the manner that I am going to explain; which at the same time will be a confirmation of what has been determined above, with respect to the *groups*.

explained.

The condition of *electric effects* in the pile is *absolutely* this: the *two metals*, either in immediate *contact*, or connected together by a *metallic* conductor; these *groups* being separated by a *non metallic* conducting substance, here the *wet cloth*: wherever therefore one of the *metals* is not in this determined connexion with the other, it does not contribute to the *electric effect* of the pile; but is an extraneous body. Now, if notice be taken of all the *piles* represented in *Fig.* 2, it will be seen, that the last piece of *wet cloth*, at the

the extremity of each of them, leaves above it one of the *metals* unconnected with the other, so that the former therefore is a mere *conductor* to the *electroscope*. Thus the *efficient gr[illegible]s* for *electric effects* terminate at and *under* that *piece of cloth*; and it m[illegible] be seen in the *figure*, that in this manner *zinc* terminates the extremity A, which is *positive*, and *silv[illegible]* [illegible] extr[illegible] b, which is *negative*: and thus, not only th[illegible]ments are reconciled, but thereby the definition [illegible] these *groups* in the *pile* is confirmed.

Without oxidation of the plates no shock produced.

Another phenomenon manifested in these experiments deserved a farther investigation, in order to avoid any mistake; as it leads to important conclusions. It has been seen, that, when the pieces of cloth were imbibed with *pure water* only, though *chemical effects* were produced in the *glass tubes*, there was no *shock*; but that this effect was produced, when the *calcination* of the *metals* in the pile was effected by the *acid* of *marine salt*. It was therefore important to know, whether this particular cause of *calcination* were an accidental, or necessary condition of the *shock*; and I thought of a way to decide it, which was, to substitute for *zinc* a metal, which, becoming *positive* with *silver*, and thus capable of producing a *motion* of the *electric fluid*, should be *calcinable* by the *acid* of *marine salt*, but not by *pure water*; I found *pewter* to be such a metal, and I made the following experiments; first by my condenser so constructed, that I could readily try the effect of small piles, placed on a *conducting* pillar, lifted up by a string against a brass ball projecting from the *receiving* plate, and which descends by its own weight when the string is relaxed.

Exp. 15. Pewter and silver.

Exp. 15. I tried in this manner a *pile* of 20 groups, *pewter* and *silver*, separated by cloth imbibed with *pure water*, and I found, that the *pewter* side affected the condenser *positively* and the *silver* side *negatively* in sufficient degrees, to give hope, that, by increasing the number of *groups*, immediate *electric signs* would be produced in the electroscopes at the extremities of the pile.

Exp. 16.

Exp. 16. I procured thin *pewter* plates the size of my *silver* plates, and I increased the number of these *binary groups*, separated by pieces of cloth imbibed with *pure water*, till I had sufficient *electric signs* at the extremities: this

was

was by 100 *groups* in each column, forming a *pile* of 200 *groups*.

1. I observed and noted the divergences in the electroscopes, *positive* at A, the *pewter* side, and *negative* at B, the *silver* side.

2. No *shock* was felt.

3. The *glass tubes* being applied, though the *electric signs* ceased at the extremities, no *chemical effect* was produced in the *water* of the tubes.

4. The condenser applied to the usual points *a*, *c*, *b*, no *residua* of *electric signs* appeared.

5. Dismounting the pile, I did not perceive any impression of the *water* on the *pewter* plates.

Exp. 17. *Exp.* 17. I mounted a pile with a sufficient number of *groups* formed of *zinc*, *silver*, and *cloth* imbibed with the *solution of marine salt*, to produce at its extremities the same *electric signs* as the above pile; which I obtained by 25 groups in each column, 50 in the whole, and the following were the results.

1. I felt the *shock*.

2. The *glass tubes* being applied, the *electric signs* disappeared at the extremities, and the usual *chemical effects* were produced in the *water*.

3. I applied the condenser to the points *a*, *c*, *b*, and found very sensible *residua*.

Exp. 18. *Exp.* 18. I mounted again the pile of 200 groups *pewter* and *silver*, but I imbibed the cloth with the *solution of marine salt*.

1. The same *electric signs* were produced at the extremimities.

2. I felt the *shock*.

3. The glass tubes being applied, the *electric signs* disappeared at the extremities, and the *chemical effects* were produced at the usual points in the *water* of the tubes.

4. Then also, the condenser being applied to the points *a*, *c*, *b*, sensible *residua* of *electric signs* were found in the usual order.

5. *Lastly*, having dismounted the pile, I found on the *pewter* plates many spots produced by *calcination*.

These

These experiments, different from those of the former set, having clearly decided the above question concerning the condition of the *shock*, the whole together ascertains the following facts.

General conclusions.

1. When there is no *calcination* produced on the *metals* of the pile, though the *electric fluid* be put in *motion*, no *chemical effect* is produced in the *water* of the tubes.

2. When that *calcination* is produced by *pure water*, though these *chemical effects* take place, there is no *shock*; the latter requiring the *calcination* to be produced by an *acid*.

3. When the *electric fluid*, in pervading the pile, is rendered capable of producing either one, or both of these effects, its course is *retarded* through the *water* of the glass tubes, and more in the latter case.

The last of these facts leads to the following conclusion. When the *electric fluid* pervades a pile wherein the *calcination* of some *metal* is going on, the *fluid* itself undergoes some *modification*, which is the cause of the concomitance of these phenomena, a *retardation* of its course through the *water*, and the production, with a very small quantity, of *effects* which it cannot produce but with an incomparably greater quantity, when set in motion by any other known means. But before I come to this object, it is necessary to ascertain an essential point, which so far I have only mentioned, namely, that the *motion* of the *electric fluid* in the pile does not depend on a separation of the *binary groups* of *metals* by a liquid, or a *wet body*; but requires only that the separation be produced by the best *nonmetallic conducting substance*; and with the leading experiments concerning this object I shall conclude this *first Part* of the *analysis*.

Presence of a fluid not necessary.

Exp. 19.

Exp. 19. I mounted the pile of 76 *groups* of *zinc* and *silver*; but instead of *wet cloth* to separate them, I used new *cloth*, which had stood some time in my room, my *hygrometer* being at about 40°.

1. *Electric signs* were produced at the extremities of the pile, but weaker than when the pieces of *cloth* were *wet*.

2. The *glass tubes* being applied, these *signs* ceased, but there was no appearance of *chemical effects* in the *water* of the tubes.

Judging

Judging that the diminution of the quantity of *electric fluid* in motion proceeded from a want of *conducting* faculty in the *wool* itself, and aware, that no *metallic* substance could be substituted for obtaining a better *conductor*, I undertook a long series of experiments, by forming piles of 20 groups *zinc* and *silver*, separated by all the substances of the *vegetable* and *animal* kinds that I could devise, applying these groups to the condenser. Of these experiments, however, I shall only mention the practical result, which was, that among all these bodies, *writing paper* was one of the best for the intended purpose, at the same time that it is the most easy to manage; and I made the following experiment.

Writing paper one of the best substances for separating the metals.

Exp. 20.

Exp. 20. I mounted again the pile of 76 groups *zinc* and *silver*, and separated them with pieces of *writing paper*.

1. I found a great increase in the *electric signs* at the extremities of the pile, comparatively with the *cloth*.

2. These *signs* ceased when the glass tubes were applied, but still no *chemical effect* was produced in the *water* of the tubes.

This experiment opened before me a new and extensive field, in which I have ever since travelled, as will be seen in the *second Part* of this analysis, and in a following paper.

X.

Hints on the Subject of Animal Secretions. By EVERARD HOME, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry**.

Animal secretions perhaps

THE brilliant discoveries of Mr, Davy on the powers of electricity in producing chemical changes suggested to me the

* Philos. Transact. for 1809, p. 385.

Dr. Wollaston's observations, inserted in the Philosophical Magazine, were published after this paper had been laid before the Society. I was

the idea, that the animal secretions may be produced by the same means. To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this Society, the object of which is the improvement of Animal Chemistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking. It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves. produced by elecricity.

The Voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and defending themselves, and therefore not Galvanic battery in the torpedo and electrical eel

I was led to the present investigation, while preparing my lectures on the Hunterean Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated, that I should bring them forward in my lectures; and at that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, Professor of Chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced.

"Trace all the nerves leading to any secretory organ in a living animal, and divide them, being careful to injure the blood-vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle the nerve of which has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy, while this paper was in the press.

immediately

abounds with nerves.

immediately applicable to the present inquiry, yet it furnishes two important facts, one, that a Voltaic battery can be formed in a living animal, the other, that nerves are essentially necessary for its management; for in these fish the nerves connected with the electrical organs exceed those that go to all other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy, to adapt them to receive and preserve a small electrical power.

Nerves and muscles form such a battery.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating electricity, is proved by the well known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off; laying bare the crural nerves; applying one of these to the exposed muscles of the other limb; and then, when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

Circumstances in the structure of the nerves not applicable to sensation.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and the uses of which have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs; the ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles; and the course of the nerves of the viscera, which keep up a connexion among themselves in so many different ways.

Blood vessels of the secretory organs do not account for their actions.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

Experiments

With a view to determine how far any changes could be

produced

produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

On the action of electricity on the blood.

Experiment 1. Middle of January, 1809.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been drawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood, which had surrounded the negative wire, was of a deep red colour, and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

Blood drawn and exposed to electricity.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment 2. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it in the longest possible time in that state.

Blood exposed to electricity in a vein.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the mesocolon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures,

and

containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, except part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed in the following experiments.

Experiment 3. 10th of March, 1809.

Serum exposed to a high electrical power,

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment 4, 14th of April, 1809,

to a low power,

Was undertaken to ascertain the effect of a low power; a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

Experiment 5. 6th of May, 1809.

and to a very low power.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very weakly charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The

(The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, [illegible] salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form, with a less degree, it is separated in a fluid form. Results.

From these facts the following queries are proposed.

1st. That such decomposition of the blood by electricity may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power. General conclusions.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose, and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron

iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, while the production of acid and alkali, effected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the Society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XI.

On the Saccholactic Acid, and its Conversion into Succinic: by Mr. TROMMSDORFF *.

Process for procuring saccholactic acid.

THE process I employed for procuring saccholactic acid differs from that described by Scheele in requiring a smaller proportion of nitric acid. Into a large glass retort put one part of sugar of milk finely powdered, and three parts of nitric acid of the specific gravity of 1·28. A receiver being adapted to the retort, heat is applied, till the liquid boils. It is then removed from the fire; and, as soon as the ebullition and the evolution of gas have entirely ceased, it is placed on the fire again. When the liquid, which is kept boiling, begins to grow thick and turbid, the retort is taken from the fire, and suffered to cool. The white powder is then separated from the supernatant liquid, which is evaporated slowly, after adding to it the nitric acid, that had passed over into the receiver. In this way a considerable additional portion of saccholactic acid is formed, which is mixed with the other. The whole of the pulverulent acid is shaken repeatedly in a vessel of cold water, and separated by filtration. In this way from 16 ounces of sugar of milk four ounces of saccholactic acid are obtained, and from the mother water about two ounces of oxalic acid.

* Abridged from Annales de Chim. vol. LXXI, p. 79. Extracted from Trommsdorff's Pharmaceutical Journal by Vogel.

A. 1. Two hundred gram. [3088 grs.] of saccholactic acid were introduced into a glass retort, to which a tubulated receiver was adapted, and from this a tube proceeded to the pneumato-chemical apparatus. The retort being placed on the naked fire, the saccholactic acid began to swell, grew black, and soon entered into fusion. First a white liquid passed into the receiver, then a yellow, accompanied with a heavy white vapour, and followed by a small quantity of empyreumatic oil. Carbonic acid gas was evolved, with a little carburetted hidrogen gas. The mixture of these gasses had a peculiar aromatic smell, by no means disagreeable. At the end of the distillation, about 60 gram. [926 grs.] of a transparent brown liquid were found in the receiver, and a yellowish crystallization. Neither the gaseous nor liquid products ever furnished any nitrogen. In the retort was a light coal, of a lustre almost metallic, which afforded some traces of lime when incinerated.

Subjected to dry distillation.

Coal

This same coal was treated repeatedly with boiling nitric acid. Being then heated with caustic soda, it dissolved in it, and produced a blackish brown liquid. Nitric acid threw down a deep brown precipitate from this solution. As the coal had considerably increased in weight, the author supposes he had formed the oxided coal of Proust.

treated with nitric acid & dissolved by soda.

2. The brown distilled liquid was poured off from the crystals, and mixed with water, which separated from it a black empyreumatic oil: it reddened infusion of litmus; and had a pungent empyreumatic smell, but not ammoniacal. Being saturated with soda, neither prussic acid nor ammonia was evolved from it by heat. The liquid, being evaporated to dryness, was in part acetate of soda; but the base was probably saturated by some other acid also. The neutral solution was precipitated by a great number of metallic and other salts; as the nitrates of silver, mercury, copper, and lead, the muriates of iron and barytes, the sulphate of manganese, and the acetate of lime.

Liquid in the receiver.

3. The crystallized mass sublimed to the top of the retort had the following properties.

Properties of the sublimed acid.

a. Heated in a silver spoon over the flame of a candle, it melted, and evaporated entirely, leaving scarcely a trace of charcoal. The vapour was inflammable.

b. Alcohol

Alcohol dissolves it without heat. After the evaporation of the alcohol, bundles of acicular crystals were left. If the matter be heated to fusion, and then left to cool, it forms a radiated crystallization, covered on the surface with small white needles.

c. Cold water dissolves this substance with difficulty, but it is easily dissolved in four times its weight of hot water.

d. The taste of this aqueous solution is perceptibly sour, and very different from benzoic acid. It has besides no analogy with the benzoic acid.

e. The aqueous solution was accurately saturated with soda; and a solution of muriate of iron at a maximum formed in it a dirty brownish red precipitate.

f. By way of comparative experiment, a solution of succinate of soda was poured into a solution of muriate of iron. The appearance was the same, except that the colour of the precipitate was less dirty, which may be attributed to some empyreumatic oil having remained in the former acid.

Similar to succinic acid.

All the results that had been obtained indicate a great analogy with succinic acid; but for more certainty it was purified, by washing with water the most perfect crystals, which were then dissolved in alcohol, and evaporated to dryness. Very pure succinic acid was employed comparatively with these.

Both gave the same results on exposure to heat,

a. Ten parts of the purified acid were exposed to a gentle heat in a phial over a lamp. When the acid was melted, a white smoke arose, which was deposited on the sides of the phial in fascicular flocks of a snowy whiteness. The phial being removed from the lamp, fine crystals an inch long sublimed in cooling. A little coal was left at the bottom, which when incinerated was found to contain potash.

aa. Ten parts of crystallized succinic acid, being treated in the same manner, exhibited exactly the same phenomena; and at the end of the process the two phials could not be distinguished from each other.

saturation with soda,

b. One part of the acid was accurately saturated with soda; and the same was done with succinic acid and soda. The quantity of water in these two salts with base of soda was the same.

c. The

c. The saturated solution did not precipitate acetate of lime, sulphate of manganese, nitrate of copper. acetate of lead, or nitrate of lead. The succinate of soda exhibited the same phenomena. examination with various tests,

d. It precipitated the nitrate of barytes white, nitrate of nickel applegreen, sulphate of cobalt peachblossom colour, nitrate of mercury white, muriate of iron at a maximum brown. The succinate of soda comported itself in every instance in the same manner.

e. Three parts of nitric acid were evaporated from one of this acid. It was not decomposed, being only rendered whiter and more pure. and treatment with nitric acid.

Succinic acid treated with nitric exhibited the same results.

To demonstrate the difference between this acid and the benzoic, I saturated twenty parts with soda, and dissolved the salt in 120 parts of water. Muriatic acid poured into it did not render it in the least turbid, but next day a solid crystallization was formed. It differed from the benzoic acid,

The benzoic acid was saturated and dissolved precisely in the same manner. The first drop of muriatic acid began to separate the benzoic in a curdy form, and a larger quantity converted the whole liquid into a light bulky mass.

From these experiments it follows, that the acid separated from the products of the saccholactic by distillation is not the benzoic.

This acid is volatile, and crystallizable; it cannot therefore be compared with the acetic, oxalic, suberic, malic, citric, or tartaric. Neither has it any analogy with the prussic, uric, or sebacic. There remain then only the gallic, benzoic, mellitic, moroxylic, pyrotartaric of Rose, and succinic, to which it can be analogous. and from all others,

It does not resemble the gallic, for it does not precipitate iron black, and when combined with soda acts neither on copper nor on lead. The benzoic acid is much less soluble in water, and the benzoate of soda precipitates almost all metallic salts. The mellitic acid comports itself very differently, according to Klaproth, with metallic solutions. The same may be said of the moroxylic and pyrotartaric.

except the succinic.

But to the succinic acid it is perfectly similar in taste, solubility, &c.

The liquid in the receiver examined.

The brown liquid, that passed over into the receiver with the sublimed acid, still remains to be examined. When it is saturated with soda, and slowly evaporated, a reddish brown saline mass is obtained. Part of this being mixed with concentrated sulphuric acid, no smell of acetic acid was emitted. The remainder of this neutral mass being dissolved in boiling water and filtered, a thick, black, empyreumatic oil remained on the filter. The filtered liquid was precipitated by acetate of lead, and this precipitate was decomposed by sulphuric acid. The liquid separated from the sulphate of lead had the following properties.

Properties of the acid it contained.

1. It precipitates the acetate of lead white.

2. By saturation with carbonate of potash, its colour is rendered deeper.

3. The neutral solution forms with muriate of lime, after some time, a precipitate, that has a saline aspect.

4. With nitrate of barytes it gives a slight precipitate, soluble in nitric acid:

5. With the nitrate of silver, or of mercury, a copious precipitate:

6. And with the nitrate of copper a dirty green precipitate:

The pyrotartaric.

These experiments seem to show, that the acid liquid contains the pyrotartaric acid of Rose.

Recapitulation.

General conclusions.

The saccholactic acid appears to contain carbon, hidrogen, and oxigen; but no nitrogen, for during its distillation in the dry way neither ammonia nor prussic acid is formed.

The saccholactic acid is decomposed by dry distillation, and yields succinic acid, pyrotartaric acid contaminated with an empyreumatic oil, a little acetic acid, and a great deal of carbonic acid gas, with a very small quantity of carburetted hidrogen gas.

It is probable, that the succinic acid contains a larger quantity of hidrogen, and less carbon, than the saccholactic acid. The latter does not inflame, till the moment it begins to be decomposed, while the succinic acid is very inflammable.

flammable. The succinic acid seems to approach nearer to the nature of volatile oils, while the saccholactic has more analogy to the tartarous acid and sugar.

The property observed in residuary coals by Proust, that, when treated by nitric acid, they became soluble in fixed alkalis, takes place also with the coal of saccholactic acid.

There is a difference between the coal of saccholactic acid, and that which is produced by the sublimed acid: the first yields lime on incineration; the second, potash.

XII.

Letter from Mr. HASSENFRATZ, *Chief Engineer and Professor of Mineralurgy at the Practical School of Mining, to Mr.* BERTHOLLET, *on the Oxidation of Iron* *.

I HAVE long been of opinion, that oxigen combines with iron in various proportions. The experiments of Proust †, by which he shows, that only two precipitates are formed in all the solutions of iron, one of green oxide and the other of red, made me waver in my opinion, and I was on the point of giving it up; but, on comparing the quantity of oxigen mentioned by Proust as combined with iron with the quantities deduced from experiments made by other chemists, who enjoy a well deserved reputation, I found such differences in the proportions, that I resolved to suspend my judgment till fresh facts should confirm or refute this doctrine.

Oxigen supposed to combine with iron in different proportions, but this denied by Proust.

In fact Proust says, that the green oxide is composed of 28 parts of oxigen to 100 of iron, and the red oxide of 48 of oxigen. The experiments of Lavoisier, those which you undertook in conjunction with Messrs. Monge and Vandermonde, those of Vauquelin, and lastly those of Bucholz ‡, prove, that iron oxided at a minimum contains 30 of oxigen to 100 of iron. As to the red oxide, chemists are not

Different proportions assigned to the oxigen.

* Annales de Chimie, vol. LXVII, p. 309.

† Memoir read to the Institute in the year 5.

‡ Annales de Chimie, vol. LXV, p. 202; or Journal, vol. XXV, p 353.

agreed on the proportion of oxigen it contains, some making it 42, others upwards of 50.

They may vary progressively from the least to the greatest.

Your immortal Chemical Statics appeared. The opinion you there advance, that "the proportions of oxigen may vary progressively from the term at which combination becomes possible to that at which it attains its highest degree," admits, like that of Proust, a maximum and minimum of oxidation, but it differs from him in two respects: he asserts, that these two points are the only ones, and he even fixes them; while you, in addition to the progressive variation of oxigen, leave these two extremes undetermined. Proust strenuously opposed your opinion;* and I must confess that his reasons, and the facts by which he supported them, staggered me. Thus I was on the point of giving up the oxidation of iron in various degrees, when I read Thenard's Considerations on the oxidation of metals in general, and that of iron in particular †, in which that able chemist appears to adopt a mean opinion between Proust's and yours; and where he shows, that there is a third oxide of iron, white oxide, which contains less oxigen than the green minimum oxide of the chemist of Madrid. These, with your answer to Proust ‡ and Darso's|| inquiry concerning the oxidation of iron, in which he says he has obtained by calcination oxide of iron containing as far as 56 of oxigen to 100 of iron, still kept my opinion in suspense.

Four oxides.

If the facts related by Thenard and Darso be true, there must be four oxides of iron: 1, white; 2, green; 3, red; 4, beyond the red as far as 56 of oxigen to 100 of iron.

Thenard.

As you have quoted the experiment of Thenard, and this chemist has the reputation of being accurate in the facts he announces, I am inclined to trust his results.

Darso.

As to that of Mr. Darso, it appears to me the more extraordinary, as all the precise experiments hitherto made had not carried the maximum oxide of iron farther than 42 or 45 of oxigen to 100 of iron, and even Proust made it no higher than 48.

* Journal de Physique, an. 1804, tom II, p. 330.

† An. de Chim. vol LVI, p. 57: or Journal, vol. XIV, p. 224.

‡ Journal de Physique, an. 1805, tom. II, p. 356.

|| Journ. de Phys. an. 1806, tom. II, p. 291: or Journal, vol. XVII, p. 221, 267, 328.

Doubting

Doubting the possibility of fixing the limits of oxidation, and obtaining an oxide containing so large a proportion of oxigen, I imparted to you my thoughts on the subject. You hinted to me, that you had little confidence in the results announced by Mr. Darso; and, as I thought it of importance to the opinion concerning the different terms of oxidation of iron to verify the fact, I sought an opportunity of doing this.

The author's lectures on mineralurgy.

The lectures on mineralurgy, which I give every year at the school of Mining at Moustier, enabling me to make a number of chemical experiments, which are repeated by the pupils sent thither by government, who have already acquired considerable knowledge of the subject at the Polytechnic school, I determined this year to repeat the experiments of Mr. Darso, and employ Mr. Desroches, a pupil, to do the same.

Darso's experiments repeated.

Following as exactly as possible the process described by Mr. Darso*, we endeavoured to perform it with more care and attention, in order to obtain results, on which others might depend.

The iron filings first analysed.

As it was possible, that some of the substance of the test, the cupel, or the mortar, might have mixed with the iron, and contributed to the increase of its weight, we first analysed the iron filings, which we proposed to calcine. By this analysis we found, that the iron we calcined contained a little carburet of iron and silex, but in too small quantity to be weighed, with a trace of alumine and of lime.

Process described.

The iron filings, weighed separately, were put into a porcelain capsule, weighed with the capsule, and then placed under the muffle of a cupelling furnace. After having calcined them for a larger or shorter time, the capsule was withdrawn, and set to cool under a glass, that no extraneous matter might get into it; after which they were weighed with the capsule, triturated, and then weighed with the capsule afresh. These operations were repeated nine times following, and the iron increased 42·224 per cent, after which it received no farther increase.

* Certainly not at the commencement, for the filings were not stirred, nor was the air renewed, till after nine successive calcinations of several hours each. T.

I shall

I shall here subjoin a tabular view of the nine successive calcinations, that the reader may observe their progress.

Tabulated results.

	Gram.	Gain.	Loss.
The iron filings weighed 5 grammes* and the capsule	29·340		
Capsule and filings	34·340		
After a calcination of 1½ hour	34·885	0·545	
After trituration	34·885		0·000
After a 2d calcination of 2 hours	35·100	0·215	
After trituration	35·090		0·010
After a 3d calcination of 3 hours	35·530	0·440	
After trituration	35·520		0·010
After a 4th calcination of 4 hours	36·380	0·860	
After trituration	36·370		0·010
After a 5th calcination of 6 hours	36·420	0·050	
After trituration	36·405		0·015
After a 6th calcination of 4 hours	36·405	0·000	
After trituration	36·390		0·015
After a 7th calcination of 3 hours	36·390	0·000	
After trituration	36·375		0·015
After an 8th calcination of 2 hours	36·375	0·000	
After trituration	36·360		0·015
After a 9th calcination of 1 hour	36·360	0·000	

Process of the calcination irregular.

From these experiments it follows, that the increase of weight by calcination was somewhat irregular in its progress, and after the 5th calcination, which was continued for 6 hours, the oxide increased no more in weight, to whatever temperature it was exposed.

Loss by trituration.

It appears too, that in all the triturations, except the first the oxide of iron lost weight, owing to that evaporation which is unavoidable, when we triturate a very fine substance, and this evaporation appeared to follow a law depending on the fineness of the particles of the oxide. At first, while the particles were still coarse, it was nothing when they began to grow fine, it was 0·010; and when they were still finer, it was 0·015.

* 77 grains English.

Calculation

Calculating the increase of weight in each calcination, and the losses of each trituration, we find, that, after five successive calcinations, the iron increased 42·224 per cent: by this process therefore we cannot carry the oxide of iron farther than 42¼ of oxigen to 100 of iron. Only 42·224 of oxigen to 100 of iron.

To satisfy ourselves whether any foreign matter, beside the oxigen had combined with the iron, we analysed the oxide. This analysis, like that of the filings, yielded us an imponderable quantity of silex, and a trace of lime and alumine. Hence it follows, that no earthy substance had combined with the iron, and that the metal had lost during calcination the carbon it contained before. No foreign substance present, but the carbon lost.

As Mr. Darso says, that he was constantly stirring his oxide of iron during the calcination, and renewing the air in contact with it by means of a pair of bellows; and as it was possible these two causes might have contributed to the combination of a greater proportion of oxigen with the iron, we resumed the experiment where we left off, and continued the operation on 3 grammes, at 42¼ of oxigen per cent, which we had left, stirring and blowing on them. As it was possible however, if we used an iron hook to stir the oxide, that this hook might have been oxided, and part of its oxide mixed with that of the filings, so as to increase its weight, we fixed on the extremity of an iron rod a hook of glass, which we changed as often as it appeared to grow soft from the heat; and with this substance we stirred the oxided iron. The oxide stirred and blown on during calcination

The three grammes of iron oxide were exposed seven times successively to the heat of the furnace under a muffle, and afterward triturated. The muffle was heated to the highest degree, the oxide was stirred during the calcination, and the air in contact with its surface was constantly renewed by means of a pair of bellows. In the earlier operations the oxide increased in weight; so that at the fourth there were 45 of oxigen to 100 of iron: but when it had reached this point we could not combine any more oxigen with it, so that it remained constantly at 45. gained 2·776 more of oxigen in 4 calcinations, but would not take up more.

These, Sir, are the results we obtained by repeating the experiments of Mr. Darso, and they confirm the opinion, that the oxidation of iron at a maximum does not exceed

45 of

45 of oxigen to 100 of the metal. It is true Mr. Bucholz could not combine more than 42 parts of oxigen with 100 of iron, as we did in our first experiment: but mine-engineer Gueniveau combined 44 parts of oxigen with 100 of iron in an experiment similar to that of Bucholz, as he informed me in a letter I received from him. However, as it is extremely difficult to have pure iron, exempt both from carbon and oxigen, we should consider the highest result as nearest the truth.

Bucholz.

Gueniveau

Darso's experiments apparently erroneous.

This difference in the quantity of oxigen combined with iron in the experiments of Mr. Darso and ours leads us to believe, that some substances mixed with his products, of which he did not take account. Our results bring back the question to the point at which it was before the experiments of Mr. Darso. If the precipitate obtained by Thenard be really a white oxide with less oxigen than the green, there must be three oxides of iron the white, green, and red: to the nature and degree of oxigenation of this precipitate therefore our inquiries should be directed, to determine whether, as Proust asserts, there be but two degrees of oxigenation; one at a maximum with 45 of oxigen, and the other at a minimum with 30.

The white oxide still remains to be examined.

XIII.

On the muriate of Tin; by Mr. E. BERARD, *Exprofessor of Chemistry at the Medical School at Montpellier, of the Academy of Garth, &c.**

Preparation of muriate of tin in the great.

THE preparation of muriate of tin in a large way has furnished me with opportunities of observing some facts, which I think it may be of use to record. They will form a supplement to those, that have been described by various men of learning, and may serve to hasten the period, at which the art of dyeing will receive from chemistry a certain and uniform method of preparing and employing the *composition* for scarlet, which is a species of salt of tin.

* Annales de Chimie, vol. LXVIII. p. 78.

The

Solution of tin by muriatic acid assisted by heat.

The solution of tin by muriatic acid, as directed by various authors, and as practised by Baumé, is effected by pouring on one part of this metal, in a state of extreme division, four parts of common muriatic acid, and assisting the chemical action by the heat of a sand-bath. The water serving as a vehicle to the acid is decomposed; the oxigen oxides the metal, which then combines with the acid; while the hidrogen is evolved in the state of gas, carrying with it some particles of the metal employed, which render it very fetid. But the action is slow, and the dissolution is imperfectly effected. I have observed, that a very large portion of the acid is completely lost by evaporation, and that, if you would dissolve the whole of the metal, you must not only add fresh acid, to supply the place of what is thus wasted, but keep up the action by artificial heat for several days. I tried to effect this operation in the cold, and two months were insufficient. Bayen and Charlard, in their experiments on tin, employed as much as six months.

Tin put into water, and muriatic acid gas passed into it.

Mr. Chaptal assists the chemical action between muriatic acid and tin, by placing the metal, when he prepares the acid, in the jars of Woulfe's Apparatus, in which is the water to absorb the vapours. The heat that is evolved has an excellent effect, and the action becomes very brisk toward the end of the process. But this ingenious device leaves something still to be desired, as the acid dissolves only a fourth of its weight of tin, and the solution requires to be finished by other means.

Tin exposed to nascent muriatic acid gas.

The solution of tin is still better effected by admitting into a large receiver, in which there is a sufficient quantity of the metal in a state of division, the vapours of muriatic acid evolved from a mixture [illegible] muriate of soda and sulphuric [illegible] for acids. In proceeding thus by simple [illegible] vapours of muriatic acid are pretty easily condensed and combined with the tin.

Tin in muriatic acid exposed to oximuriatic acid gas.

If the vapours of oximuriatic acid be received into a vessel containing tin and common muriatic acid, the solution is effected completely, and in a short time. The acid at 20° will then take up the third of its weight of tin.

Action of ni-

I have tried various mixtures of muriatic and nitric acid, containing

tromuriatic acid on tin.

containing from a sixth to a tenth of the latter. They all acted on tin with extreme heat and violence, and the contents of the vessel were thrown out with violence. One part of nitric acid, or aqua fortis of the shops, at 35° of Baumé's areometer, and 12 parts of common muriatic acid at 20°, form a mixture well adapted to the solution of tin, which it effects very well, and in little time. This mixture takes up about a third its weight of tin, and the solution is carried to 45°.

Tin exposed alternately to muriatic acid and atmospheric air.

I attempted to combine the alternate action of muriatic acid and atmospheric air on tin, divided into small grains, for the purpose of dissolving it, and with complete success. With this view I filled a large wide mouthed glass bottle with finely granulated tin, covered the metal with muriatic acid at 20°, left this to act on it for a few hours, and then poured off the acid into another vessel; when it was found to have risen to 25°. The tin soon began to grow black from the contact of the atmosphere, absorbed oxigen from it, and caloric was evolved, rendering the metal very hot. A lighted candle put into the bottle was quickly extinguished. As soon as the bottle began to grow cool, I returned the acid into it, which acted with fresh force, and in a little time got to 35°. I poured it off again, to let the air act on the tin, and then returned it into the bottle afresh. Thus I continued proceeding alternately, till all action ceased. At the end of two days the solution had attained the strength of 45°. Indeed one day was sufficient for this, if a series of bottles supplied with tin were employed, so that the acid might be acting on the tin in some, while the air was acting on that in others; and by this continual action the strength might be carried even to 50°.

Solution of muriate of tin readily absorbs oxigen from the air,

The muriatic solution of tin, when fresh made, combines pretty readily with the oxigen of the atmosphere, as Pelletier, Guyton Morveau, and other celebrated chemists have observed. It is sufficient to invert a jar, filled with atmospheric acid, over a dish or widemouthed bottle filled with this solution, when the solution will continue to rise in the jar, till the whole of the oxigen is absorbed. The absorption is more rapid if the jar be filled with pure oxigen gas; and nearly the whole of the gas will be taken up in a short time. To facilitate the combination of oxigen gas with

with the recent solution, I caused a large quantity of atmospheric air to pass through it by means of a pair of bellows, the nozzle of which reached to the bottom of the liquid. If the solution be not fully saturated with tin, it will take up a fresh quantity in proportion as it absorbs oxigen from the atmosphere.

Oximuriatic acid gas is eagerly absorbed by this solution, as Pelletier very justly observed. This learned chemist even proposed a solution so saturated for the purpose of dyeing scarlet; and I prevailed on several dyers to make trial of it, but none of them adopted its use It appears, that the combination of atmospheric oxigen with it imparts to it nearly the same properties as oximuriatic acid gas. When it has absorbed a great deal of oximuriatic acid gas, it is fit for dissolving a fresh quantity of tin, and when it has dissolved more tin, its state is altered, and it is rendered again capable of absorbing oxigen gas.

and oximuriatic acid gas, which has a similar effect on it.

The muriatic solution of tin at 45° yields crystals of muriate of tin, by evaporation. The crystallization is effected more easily, in proportion to the length of time the solution has been kept, or to the quantity of oxigen it has absorbed. The mother water, in which the crystals are deposited, is of great density, particularly after several crystallizations, Its density is still greater, if it were evaporated before its exposure to the air: it is sometimes even slightly fuming, and will then yield crystals on being diluted with pure water. A phial that would hold 14 parts of distilled water, contained 28 of the mother water after the first crystallization: and it held 31, when the same liquid had furnished several crops of crystals by evaporation. These mother waters are capable of combining with the oxigen of the atmosphere, if the solution were not previously saturated with it: and for this purpose it is sufficient to expose them to the air, or force the air through them with a pair of bellows, as I have pointed out above for the simple solution. This combination occasions a fresh production of crystals, and if a very extensive surface of the mother water be exposed to the air, a muriate of tin crystallized in very thin and light scales will be obtained. Baumé noticed this mode of crystallization. Oximuriatic acid gas combines with the mother water with much

Crystallization of the muriatic solution of tin.

much energy; a considerable quantity of caloric is evolved, and after it is cold the liquid coagulates into a mass of silky crystals of muriate of tin. If the crystals of muriate of tin be purified by dissolution in pure water and recrystallization, they will acquire more consistence and more density.

Crystal very soluble, producing much cold.

Crystallized muriate of tin is very soluble in cold water, the solution being quickly effected, and producing a considerable diminution of the temperature. The mean diminution in my experiments was 9° of R. [20·25° F.], the temperature of the atmosphere and of the substances employed being 5° [43·25° F.]. The mixture of pure water with the mother waters produced no change of the temperature.

Mother waters distilled.

Butter of tin.

Fuming liquor, weaker than that of Libavius.

As I had observed, that these mother waters became a little fuming by evaporation, I tried the distillation of the mother waters highly concentrated, and of the crystallized muriate, to see whether I should not obtain a muriate of tin similar to that known under the name of fuming liquor of Libavius. Weak muriatic acid first came over; and then the muriate either passed into the receiver, or sublimed into the neck of the retort in a white mass, known formerly under the name of butter of tin. With the same view I passed muriatic acid gas as dry as possible through the concentrated mother water of muriate of tin when it became fuming, and yielded crystals on mixing with it pure water. But I must observe, that the fuming liquor of Libavius emits much more dense and copious vapours, that it is whiter, and that its specific gravity is greater.

The muriate has always an excess of acid, and variable,

except in crystals, which should always be used for dyeing.

The combinations of muriatic acid and tin in the state of solution, of crystals, or of mother water, have always an excess of acid; and from what has been said it appears, that they are all capable of infinite variations in their state. Hence we need not be surprised, if the effects they produce in dyeing are so uncertain, and so different from one another. The least variable state of muriate of tin appears to be that of crystals perfectly white and thoroughly drained. In this state this mordant ought always to be employed in dyeing, adding to it a larger or smaller proportion of nitric acid, according to the shade we wish to produce. Such a composition alone can be always uniform, and yield constant results.

Profiting

Profiting by the facts detailed in this paper, it appears easy to give a simple and advantageous process for preparing the crystallized muriate of tin in the large way, yet I have met with very perplexing difficulties in the attempt to carry it into execution. These however I have at length been able to surmount, and the description of the method I have pursued will form the subject of a future paper. Preparation in the great.

XIV.

On the Formation of Acetic Ether in the Marc of Grapes: by Mr. Derosne*.

SOME years ago we obtained acetic ether by the simple distillation of vinegar on a pretty large scale. This year we had an opportunity of remarking its formation in the marc of grapes, from which the juice had been expressed. We were preparing some sirup of grapes, for a trial; and after the grapes had been bruised, they were subjected to the press, and the squeezed pulp was thrown into a cask. Some days after, accidentally thrusting my hand into it, I found it warm and moist; but what surprised us most was a smell of ether arising from it. Some of it was taken out, pressed, and the fluid distilled in a small alembic. The first product was in fact pure acetic ether. What came over afterward contained some likewise, but mingled with weak spirit of wine and acetous acid. Acetic ether formed in the distillation of vinegar, and from the refuse of pressed grapes.

It appears, that this marc fermented very quickly; that the acetous fermentation took place nearly at the same time with the spirituous; and that their simultaneous occurrence produced acetic ether. Theory of its formation.

Thus this liquid, which, when first discovered, seemed very difficult to be obtained, appears to be formed under circumstances of no unfrequent occurrence; and perhaps, if we opportunely availed ourselves of the moment of its spontaneous formation, we might procure it in sufficient quantity, without having recourse to its artificial formation †. Might be obtained in this way.

* Ann. de Chim., vol. LXVIII, p. 331

† In our cider countries the pomasse, or refuse of the expressed apples, might be examined for this product. C

XV.

XV.

*Late sown Clover**.

Late sown clover.

IN 1804, the war having deranged the usual course of agriculture in Saxony, a farmer, Mr. Thaer, was unable to sow his oats and clover before July. The first mowing of the clover was a very slight crop; but the second was as fine and thick as possible. Mr. Thaer has since repeated the experiment, and he has found, that clover, when sown later than ordinary, grows more abundantly, and of better quality; and that, provided it be sown before the middle of August, it will certainly succeed.

SCIENTIFIC NEWS.

German and French Dictionary of mining.

MR. J. B. Beurard, agent of government at the quicksilver mines of the late Palatinate, has published a "German and French Dictionary, containing the Terms employed in Working Mines, in Mineralurgy, and in Mineralogy, with the Technical Terms of the Sciences and Arts connected with these." It occupies one large 8vo volume, and is said to have much merit.

Mode of splitting rocks by lightning.

Last summer an experiment of a new kind was tried at the village of Philipsthal in East Prussia. This was, to split a rock by means of lightning. An irod rod, similar to a conductor, was fixed in the rock, and on the occurrence of the first thunderstorm, the lightning was conducted down the rod, and split the rock into several pieces, without displacing it.

Plants & insects of the south of France.

Mr. Philip Salzmann, of Montpellier, has published a catalogue of the plants and insects of the South of France, which he proposes to collect and send half yearly to any who may order them, at the prices annexed.

Pharmaceutical Society at Paris prize questions.

The Pharmaceutical Society of Paris has proposed the two following prize subjects for the present year.

1. To ascertain, as far as possible, whether there exist in

* Sonnini's Biblioth. Physico-écon. Oct. 1808, p. 223.

in vegetables a peculiar and distinct principle, to which chemists have given the generic name of extractive.

Ought we to retain the old classification of pharmaceutic extracts; divided, according to Rouelle, into gummy, resinous, gummy-resinous, resino-gummy, and saponaceous?

Can we establish a more methodical and accurate classification by the help of chemical experiments on the principal substances, that furnish the apothecary with extracts?

To indicate, from the nature of their different constituent principles, the mode of preparation that agrees with each of them, and the nature of the menstruums that should be employed.

2. What is the present state of pharmacy in France? What part does it take in the art of healing? and of what improvements is it susceptible?

The prize for the first is a gold medal of the value of 200 francs [£8 6s. 8d.]; for the second, a gold medal of the value of 100 f. [£4 3s. 4d.]. The answers to each are to be sent, post-free, to Mr. Sureau, secretary to the society, rue Favart, No. 18, before the 1st of October next.

Medical and chemical lectures.

Dr. George Pearson, F. R. S., senior physician to St. George's Hospital, recommences his course of lectures on Physic and Chemistry, at No. 9, George street, Hanover square, on the 4th of June, at the usual morning hours; namely, the medical lectures at 8, and the chemical at 9. A chemical lecture on the cases of patients of St. George's hospital is given every Saturday morning at 9; and the practice of vaccination is carried on in Broad street, Golden square.

To Correspondents.

Mr. Verschoyle's paper, dated the 16th of April, did not come to hand till the 26th of May. I shall be happy to receive an account of his progress, and at the same time should be glad of a more precise representation and description of his apparatus. Perhaps Articles IX and X of our present number may afford him some hints on the subject of his inquiry.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For MAY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the Strand, London.

APR. Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day.	Lowest in the Night.	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	51°	51°	59°	45°	30·13	Fair	Fair
28	52	55	62	45	30·11	Ditto	Ditto
29	51·5	56	62	47·5	30·12	Ditto	Ditto
30	53	57	65	48	30·01	Ditto	Ditto
MAY							
1	55	51	58	43	29·88	Ditto	Ditto
2	48	51	54	42	29·88	Ditto	Cloudy *
3	44	45	50	38	29·75	Showery	Fair
4	49	43·5	50·5	37	29·72	Rain	Cloudy †
5	44	43	51	33	29·86	Fair	Fair ‡
6	42	44	47·5	38	29·81	Cloudy	Ditto
7	43	47	48	45	29·68	Rain	Rain
8	51	51	59	47	29·44	Fair	Fair
9	54	55	59	50	29·79	Ditto	Ditto
10	55	55	61	46	29·96	Ditto	Ditto
11	50	48	61	42	30·10	Ditto	Ditto
12	47·5	53	57	49	30·01	Ditto	Cloudy §
13	51	50	52	47	29·87	Rain	Fair
14	51·5	53	57	50	29·67	Fair	Cloudy ‖
15	52	51	54	45	29·40	Cloudy	Ditto ¶
16	49	55	58	46	29·40	Fair	Fair
17	50	55	57	43	29·63	Rain	Rain
18	45	44	46	38	29·57	Ditto	Ditto
19	43	49	53	41	29·97	Fair	Fair
20	49	51	55	47	30·00	Ditto	Ditto
21	55	54	64	47	29·67	Ditto **	Ditto
22	55	53·5	61	44	29·86	Showery	Ditto
23	53	52	60	43	30·10	Fair	Ditto
24	52	54	62	47	30·18	Ditto	Ditto
25	54	53·5	63·5	43·5	30·08	Ditto	Ditto
26	49	53	61·5	46	30·06	Ditto	Ditto
27	53·5	54	64	46	29·95	Ditto	Ditto

* Starlight at 11. Rain in the night.
† The afternoon cold.
‡ This evening very cold.
§ Rain at 10.
‖ Rain at 11, and most of the night.
¶ Rain in the night.
** The day fair. A little rain about 7 P. M.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1810.

ARTICLE I.

Observations on the Combustion of several sorts of Charcoal, and on Hidrogen Gas: by Theodore de Saussure. *Read at the Society of Natural Philosophy and Natural History of Geneva, August the* 31*st*, 1809*.

It is well known, that the proportions of carbon and oxigen in carbonic acid gas could only be determined in a vague manner by the experiments of Lavoisier. The detail of his inquiries † shows that 100 parts of this gas might contain between 25 and 28 parts of carbon: and though he first adopted the latter proportion, he ultimately concluded, that the former was too great, and that the quantity of carbon contained in 100lbs. of the gas did not exceed 24lbs. ‡

Proportion of base in carbonic acid undetermined.

Among the inquiries into the subject since made, those of Messrs. Allen and Pepys ‖ are particularly to be distinguished. They found by the combustion of charcoal in oxigen gas, that 100 parts of carbonic acid gas by weight contained 28·6 parts of carbon, or of diamond, or of charcoal

Messrs Allen and Pepys

* Abridged from Annales de Chimie, vol LXXI, p. 254.

† Mémoires [illegible] Sciences, 1781.

‡ Lavoisier's [illegible]), vol. II.

‖ Phil. Trans. [illegible]: or Journal, vol. XIX, p. 216.

previously heated red hot in the fire; for these two substances give the same results. They assert, that dry charcoal produces no water in burning, and contains no hidrogen: but the details of their processes show, that their results can be considered only as approximations sufficiently doubtful.

burned charcoal in too small quantity.

In each of their experiments these gentlemen burned only four grains of charcoal, and it is impossible to obtain accurate results with such small quantities. They estimated the density of the oxigen gas and the acid gas by weighing merely 21 cubic inches of these gasses, and determinations of this kind should be made from much larger quantities. We cannot justly give these results a preference over those which Lavoisier, Fourcroy, Vauquelin, Seguin, and Biot obtained with fifteen and even forty times the quantity, and very sensible balances; though indeed the sensibility of a balance is a very inadequate compensation for a defect of quantity in the substance weighed.

Quantity more important than accuracy of weighing.

The experiments of Messrs. Allen and Pepys do not prove, that charcoal previously heated red hot furnishes no hidrogen gas in burning: for they did not seek for this gas in the air remaining after the combustion, and it is well known, that hidrogen gas will not inflame when it exists but in very small proportion in oxigen gas.

These considerations have induced me to make known my researches on the same subjects, the diamond excepted, which I could not burn; and they have led me to some important conclusions on the most usual eudiometrical processes.

Weight of oxigen gas and carbonic acid gas.

Weights of oxigen gas and carbonic acid examined.

One of the most important points in the analysis is to determine the exact weight of oxigen gas and carbonic acid. In a process so delicate I judged it proper, not to rely on my own experiments alone, but to take the mean of the results, that appeared to me most deserving confidence. The balance I used in the following experiments was made by Fortin. When loaded with the globe employed for weighing the gasses it is sensible to a milligramme [0·01544 of a gr.]

gr.]. The globe weighs about a kilog. [2lbs. 3oz. avoird.], and contains 5941·6 cub. centim. [362 cub. inches.]

I reduced the volume of gas to the pressure of 0·758 met. [29·82 in.], and the temperature to 12·5° [54·5° F.], admitting with Gay Lussac, that air dilates $\frac{1}{267}$ for each degree. The temperature of melting ice, to which the volume of a gas is sometimes reduced, is so remote from those at which the weighings were made, that, if there be a little inaccuracy in our method of correction for each degree of temperature, the errour must increase to a sensible quantity, when the reduction is considerable. My experiments appear to indicate generally, that, when a gas is weighed at a temperature approaching that of melting ice, and the usual calculations are made to reduce it to the mean temperature, and the extreme humidity agreeable to this temperature, the gas, thus corrected, appears sensibly lighter, than it is found to be by actual experiment at this mean temperature. On the contrary, if a gas be weighed at a temperature considerably above the mean, and reduced to this mean by calculation, the corrected weight will come out greater, than that obtained by direct experiment. Though this result requires for its confirmation a greater number of observations, it leads me to think that we ought to avoid too great corrections for temperature.

The weight of oxigen and carbonic acid gasses are reduced to the term of extreme humidity, agreeable to the mean temperature of 12·5° [54·5° F.], because we obtain them nearly in this state. The specific gravity of aqueous vapour, at temperatures very remote from the mean, has not yet been determined in a manner sufficiently precise, to allow us no room to apprehend errour in reducing it to the dry state.

Weight of a cubic decimetre [60·895 cub. in.] of humid oxigen gas, the thermometer at 12·5° [54·5° F.], and barometer at 0·758 of a metre [29·82 in.]. Weight of oxigen gas.

According to Lavoisier	1·3583 gram.	20·9725 grs.
——— Seguin, Fourcroy and Vauquelin	1·3523	20·8798
——— Biot	1·3538	20·9030
——— my observation	1·3563	20·9416
Mean	1·3552	20·9242

The

The following are the particulars of my experiment, from which the preceding weight was ded[illegible]

Capacity of the globe.	*Vacuum by the barometer of the air pump.*	*Temperatu of the gas.*	*Barometer corrected for temperature 12·5° or 54·5° F.*	*Weight of the gas in the globe without correction.*	[illegible]
5941·6 cub. cent.	0 0056 met.	18·2°		7·52 gram.	nit[illegible]
362 cub. inches.	0·22 of an inch.	64·76 F.	0·7299 met. 28·72 in.	116·11 grs.	

In another experiment the results came out

5941·6 cub. cent.	0·005 met.	5°	0·7204 met.	7·77 gram.	nitrogen 2 parts.
362 cub. inch.	0·1967 inch.	41° F.	28·34 in.	119·97 grs.	

but as these would have given the weight of the cubic decimetre, calculated as above, 1·3445 gram. [20·7594 grs.], I thought this differed too widely from the preceding to be admitted.

Weight of a cubic decim. [60·895 cub. in.] of humid carbonic acid gas, the thermometer being at 12·5° [54·5° F.], and the barometer at 0·758 met. [29·82 in.]

According to Lavoisier	1·8470 gram.	28·5181 grs.
——— Biot	1·8591	28·7049
——— ———	1·8717	28·8995
——— my experiment	1 8565	28·6648
——— ———	1·8432	28·4594
——— ———	1·8680	28·8424
Mean	1·8570	28 6815

The

The following are the particulars of the experiments here calculated from.

Capacity of the globe.	*Vacuum by the barometer of the air pump.*	*Temperature of the gas.*	*Barometer, corrected for temperature 12·5 or 54·5 F.*	*Weight of the gas in the globe without correction.*	*1000 parts of the gas contained*
5941·6 cub. cent. 362 cub. inches.	0·007 met. 0·2754 inch.	14° 57·2° F.	0·7295 met. 28·7 inches.	10·476 gram. 161·752 grs.	9·29 atmosph. air.
Ditto	0·005 met. 0·1967 inch.	5·94° 42·67° F.	0·7265 met. 28·58 inches.	10·775 gram. 166·368 grs.	6·9 nitrogen, 5·8 oxigen.
Ditto	Ditto.	5° 41° F.	0·7232 met. 28·45 inches.	10·765 gram. 166·244 grs.	9·2 nitrogen 7·8 oxigen.

Direct experiments on the density of gasses are frequently omitted: but they would be of advantage, not only to correct results, that are useless from being founded on imperfect methods, but likewise [illegible] errours of these methods.

[illegible]aratus for the combustion of charcoal.

Apparatus for the combustion of charcoal.

[illegible]n was effected in the sun, by means of a [illegible] [12·8 inch.] in diameter, in a tubulated receiver [illegible] cock, which contained about 2600 cent. cub[illegible] cub. in.]. The charcoal was fastened by a wire of platina to a plate of the same metal, which was suspended in the middle of the receiver by a chain of platina fastened to the top of the vessel. Near its point of suspension, and in the tubulure, was fastened to the chain a small cylinder filled with muriate of lime, which had been weighed in a closed vessel previous to the experiment.

The receiver was suspended between two vertical pillars by a horizontal bar fixed to the tubulure, and crossing the pillars in the direction of their diameter. This bar could be moved up and down between the pillars, and be fixed by screws; so as to keep the receiver immersed more or less in a mercurial trough, which was deep enough to fill the receiver with mercury, when plunged into it perpendicularly with the cock open. The mercury was previously dried, and the tubulure alone was left full of common air, that the mercury might not mix with the muriate of lime. Oxigen gas from the oximuriate of potash was introduced into the receiver, by fitting to the tubulure a bladder filled with this gas, and furnished with a cock, and raising the receiver to a proper height; after which the cocks were closed.

To extract the gas from the receiver, I screwed on the tubulure a small glass globe filled with mercury. On opening the cocks, the mercury in the globe fell into the receiver, and was replaced by the gas in the latter. The pipe of the cock of the tubulure had a tube leading into the receiver to convey the mercury free of the muriate of lime.

Before I commenced the process of combustion, I always extracted in this way a part of the oxigen gas that had been introduced into the receiver, and subjected it to analysis.

Eudiometrical processes. For oxigen.

To ascertain the proportion of oxigen gas, I employed the hidrosulphuret of potash, concentrated, [illegible]gnated with nitrogen gas; and I always compared [illegible] process with that

that of Volta's eudiometer. This comparison led me to several new observations on the use of this instrument.

In the trial with hidrosulphuret I followed nearly the process of Marty. I shall only observe, that I coated with suet and sand the glass stopple of the phial containing the hidrosulphuret and gas, to prevent its being completely closed. Without this precaution a vacuum is formed by the absorption of the oxigen gas, which occasions the evolution of the nitrogen that impregnates the liquid.

I let the mixture of hidrosulphuret and gas to be analysed stand at rest for five days, in which time the process is always finished. In this way I obtained more regular results, than those obtained in a few minutes by agitation according to Marty's process.

I take care, that the whole of the process is conducted at a temperature nearly uniform; for, if the hidrosulphuret be exposed to a lower degree of heat than that, at which it was impregnated with nitrogen gas before being placed in contact with the gas to be examined, it absorbs nitrogen from the latter; on the contrary, at a higher temperature it adds to it.

The eudiometrical process with the hidrosulphuret is more accurate than Volta's process, as will appear hereafter, to determine the proportion of oxigen gas when mixed with nitrogen only: but when the mixture contains carburetted or oxicarburetted hidrogen gas, it is best to employ Volta's eudiometer, or some other process in which a large quantity of water is not essential; for the liquid hidrosulphuret, or even pure water, sensibly absorbs all oxicarburetted hidrogen gasses, and makes the proportion of oxigen gas appear larger than it really is. This observation is true however only when the proportion of carburetted hidrogen exceeds one per cent of the gas analysed *.

* The absorption of oxicarburetted hidrogen gasses by hidrosulphuret of lime has been announced by Mr. C. L. Berthollet in his excellent paper on these gasses in the Memoirs of the Society of Arcueil, vol. II, p. 79. I have observed, that the olefiant gas is absorbed in equal quantities by pure water and a solution of hidroguretted sulphuret of potash; but that pure hidrogen gas is absorbed in larger quantity by pure water than by the liquid hidrosulphuret. Absorption of inflammable gasses.

To

Separation of the carbonic acid gas. ... the carbonic acid gas from the oxigen and ni... after the combustion of the charcoal, I employed ... pneumatic apparatus. I immersed the mer... small globe, which by means of the vacuum ex... gas from the receiver, where the combustion had been effected; and I passed the air contained in this globe into a wide tube filled with mercury, which was long enough to allow the column of gas to stand about 0·27 of a met. [10·6 inch.] high. I then introduced through the mercury one or two grammes [15 or 30 grs.] of highly concentrated solution of potash, agitated the solution in the gas occasionally, and in a few hours the whole of the acid gas was condensed. I then replaced the mercury by water and measured the absorption.

Separation of carbonic acid from oxigen by lime water not to be depended upon. Experience has shown me, that, when the carbonic acid gas is mixed with oxigen gas nearly pure, considerable mistakes are made in separating the two gasses by lime water. This liquid, by means of the agitation required for the complete condensation of the acid gas, absorbs oxigen gas. This effect is not produced by the lime, but by the water of the solution, which is required to be in very large proportion to the volume of gas *.

Potash preferable. Liquid potash absorbs nearly the same proportion of oxigen gas as an equal bulk of lime water; but as the solution of potash may be used in an infinitely less quantity, the errour arising from it is too small to be noticed.

Lime water gives out nitrogen, Lime water affects the result too, not merely by absorbing oxigen gas, but by replacing it by nitrogen gas, with which it is always more or less impregnated †.

but may be used when much of this is present. When the acid gas is mingled with common air, or with oxigen gas contaminated by a large proportion of nitrogen, lime water may be employed to absorb the acid gas without any sensible errour.

Experiments on the absorp... The following is the mean result of my experiments on this subject, under a temperature of 14° [57·2 F.]. The

* This affects the calculation from the quantity of gas absorbed, not that from the precipitate thrown down. C.

† This would alter the state of the residual gas, it is true; but it would tend to correct the calculation made from the quantity of gas absorbed. C.

limewater I used was prepared by mixing two handfuls of quick lime with six quarts of very pure rainwater, and filtering the solution at the end of eight and forty hours. The oxigen gas was not mixed with the limewater till it had been in contact with liquid potash. After this operation hidrosulphuret of potash indicated in it one per cent of nitrogen.

tion of oxigen gas,

Exp. 1. A hundred parts of this oxigen were shaken 60 times, during one minute, with 400 parts of limewater by measure, in Fontana's eudiometrical tube, and were thus reduced to 96 parts. On repeating the operation, these 96 parts were reduced to 92. I made the same experiment several times with common air, but no perceptible change in its purity or quantity took place.

by limewater,

Exp. 2. A hundred parts of pure oxigen gas were mixed with 900 of limewater in a bottle closed with a glass stopple. After a minute's agitation the 100 parts were reduced to 92·5; and these 92·5 contained 8 parts of nitrogen gas. Previous to their mixture with the limewater they contained but one part, and they had been in contact with a quantity of liquid potash sufficient to absorb more than 200 parts of carbonic acid gas. This experiment was repeated by mixing with limewater 100 parts of common air, which was not perceptibly altered either in quantity or quality.

Exp. 3. A hundred parts of pure oxigen were introduced into a closed phial with 900 parts of rainwater holding in solution a fourth part of its weight of pure potash. After shaking for a minute, these were reduced to 92·5 parts. I obtained a similar result with rainwater alone.

by potash in a large quantity of water,

Exp. 4. A hundred parts of pure oxigen gas were kept for six hours in contact with four parts of concentrated solution of potash. The gas was not perceptibly altered either in quality or quantity, though the mixture was shaken several times.

& by concentrated solution of potash.

When common charcoal, or wood, or an oil, or any vegetable substance of which hidrogen constitutes a part, is burned in pure oxigen gas, hidrogen gas is always found mixed with the oxigen gas after the combustion, though the quantity of oxigen gas be much greater than is necessary

Presence of carbon in hidrogen gas hitherto considered as pure.

... for burning the inflammable gas and all the combustibles. It is obvious, that the proportion of this hidrogen gas cannot be otherwise than very small: but the electric spark will not occasion its entire disappearance. The presence of this gas can be demonstrated only by the process given by Messrs Humboldt and Gay-Lussac for determining the combustion of a very small proportion of hidrogen in another gas. This process consists in adding 100 parts of hidrogen to 200 of the air to be analysed, and detonating the mixture with a given quantity of oxigen gas, which must be somewhat more than sufficient for burning the hidrogen. If this detonation cause a greater diminution than would result from burning the 100 parts of hidrogen added, it is to be concluded, that the gas analysed contained hidrogen gas. On this subject I shall make a few observations, to which it is of importance to attend.

The residuum of the detonation should be treated with potash, to find whether the combustion of the hidrogen gas that may be discovered furnished any carbonic acid, that no sensible quantity of carbon in the analysis may be neglected: but the estimation of the acid gas thus formed cannot be accurate, unless we deduct the carbonic acid, which the hidrogen gas used as a reagent, and deemed pure, always furnishes by its combustion, when this is effected with a surplus of oxigen gas.

Experiments on hidrogen gas obtained in various ways.

I have found to my surprise, that every kind of hidrogen gas supposed pure furnishes, when completely burned, a sensible quantity of carbonic acid gas. I have tried hidrogen gas obtained from the purest iron by means of sulphuric acid diluted with distilled water; that obtained by a similar process from zinc purified by sublimation; that from solution of tin by muriatic acid; that from the decomposition of ammonia in a redhot tube; and lastly, that from the decomposition of distilled water by Volta's pile, using platina conductors, and arranging the apparatus so that only mercury, platina, and glass were in contact with the water, and with the gasses produced. Each of these kinds of hidrogen gas, when burned with an excess of oxigen, always produced a gaseous residuum, which was partly absorbed by potash, and rendered lime water turbid by forming

forming carbonate of barytes. The gasses were always left to stand on potash previous to the detonation; a precaution particularly necessary for the oxigen gas I employed, and which I obtained from oximuriate of potash. This gas is never free from a few thousandth parts of its bulk of carbonic acid.

Results.

A thousand parts of hidrogen gas obtained from the solution of zinc purified by sublimation, when detonated with 1000 parts of oxigen gas, afforded 3 parts of carbonic acid gas.

Hidrogen gas obtained from the zinc of the shops, which had not been sublimed, produced the same quantity of acid as the preceding.

A thousand parts of hidrogen gas from the solution of iron produced 4·5 of acid gas in a similar process.

The hidrogen gas from solution of tin in muriatic acid afforded 9 parts of carbonic acid.

That from the decomposition of water by Volta's pile, 3 parts.

That from the decomposition of ammonia, 10 parts.

To render such small quantities of carbonic acid gas sensible, I effected the absorption over mercury in a tube of such length, that the column of gas I examined was 7 dec. [27·5 inches] high, and 13 mil. [0·5 of an inch] in diameter. The process was not finished in less than 24 hours.

No nitrous vapour affected these.

It may be supposed, that the diminution of the gas by the potash was owing to the absorption of nitrous vapour, which might be formed in these combustions where a small quantity of nitrogen is always present: but I constantly made the detonation over an extensive surface of water, and left the residual gas in contact with this fluid for an hour, or a sufficient time to prevent any suspicion of the presence of this vapour.

Barytes water let fall a carbonate.

I have said, that, when barytes water was employed instead of potash, the earth was precipitated in the state of carbonate. Not to be deceived in a result so small, I introduced two ounces of an aqueous solution of barytes with excess of water into a globe of thick glass, the capacity of which was 754 cub. cent. [46 cub. inches.]

This

Experiment.

[illegible] was furnished with a cock, and two conducting wires. After having exhausted it of air, I detonated in it a mixture of equal parts of oxigen gas and hidrogen gas obtained by dissolving sublimed zinc in sulphuric acid. After the acid gas had been absorbed by the barytes water, I extracted the residuary gas by means of the airpump, and introduced a fresh mixture of gasses similar to the former, which I detonated. These operations I repeated, till I had effected the complete combustion of 2·91 cub. dec. [177 cub. inches] of hidrogen gas at 0·73 met. [28·7 inches] of the barometer, and 18·75° of the thermometer [65·75° F.]. The carbonate of barytes formed weighed 6 cent. [0·926 of a gr.]. Admitting with Klaproth, that 100 parts of carbonate of barytes by weight contain 22 of carbonic acid, we shall find, that the 1000 parts of hidrogen gas by measure formed on combustion 2·6 of carbonic acid. This comes sufficiently near to the quantity deduced from the absorption by potash, to allow the two results to be considered as nearly similar.

Less carbonic acid obtained by the French chemists.

I enter into these particulars, because the quantity of carbonic acid, which the French chemists obtained from the combustion of hidrogen gas extracted by means of zinc, in their grand experiment on the composition of water *, was equal only to a thousandth part of the hidrogen; while in my experiments the quantity of this acid was about three times as great, whether the zinc I employed were purified or not, or the hidrogen gas were obtained by any other method and considered as pure.

Water from the combustion of oxigen and hidrogen said to precipitate limewater.

In the first disputes on phlogiston and the nature of hidrogen gas, some chemists announced, that the residue of the detonation of oxigen and hidrogen gasses precipitated limewater. Cavendish and Lavoisier carefully repeated this

* These gentlemen agree, that they could only estimate by approximation the quantity of hidrogen gas in the 987 cubic inches left after the combustion; but they estimated it at no more than a sixteen thousandth part. It appears probable to me, that it was more considerable, since phosphorus was capable of producing a d[illegible]. I believe too, that this hidrogen gas was highly oxicarb[illegible]

experiment

experiment, but did not observe the precipitation*. I have found, however, that these opposite results may be reconciled in some degree, by attending to the proportions of the oxigen and hidrogen gas. If these gasses be mixed in such proportion, that the hidrogen predominates after the combustion, scarcely any acid gas is formed, because the greater part of the carbon remains dissolved in the hidrogen gas unburned; and the latter is proportionally more oxicarburetted, than it was before the detonation. But when the combustion is effected with excess of oxigen gas, almost all the oxicarburetted hidrogen is burnt, and a sensible quantity of acid gas is obtained. Thus when I detonated 1000 parts of the purest hidrogen gas with 1000 of oxigen, I obtained at least 3 parts of carbonic acid: but I obtained only one part of this acid, when I detonated 1500 parts of the same hidrogen with 500 parts of oxigen. In the latter case the superfluous hidrogen was found to have a larger proportion of carbon in an equal volume, than before the detonation.

This depends on the proportions of the gasses.

These results are interesting, as they prove, that we do not yet know the density of pure hidrogen gas, and that what we consider as such always contains a small proportion of carbon, and probably of oxigen; since Mr. Berthollet has found, that no pure carburetted hidrogen gas exists, those so considered always containing some oxigen †. It may even be questioned, whether carbon and oxigen be not essential to the constitution of the substance which we have improperly termed pure hidrogen gas. These results too indicate, that the purest distilled water contains carbon; and that the same may be said of ammonia, though the carbon is in very small proportion, and perhaps accidentally pre-

Pure hidrogen gas not yet known.

Carbon in distilled water, & in ammonia.

* At that time it was not doubted, that the precipitation observed was owing to the impurity of the metals, which had been used for procuring the hidrogen. Hence the existence of charcoal in zinc was admitted. Proust however could find no carbon in the black residuum left by this metal after its dissolution in acids. To me it appears very probable, that the carbon of the hidrogen gas evolved by zinc, as well as by several other metals, is to be ascribed in part to the water decomposed.

† Mém. de la Société d'Arcueil, tom. II.

sent

[illegible] two substances. It may act an important part however in vegetation, and in a great number of other processes.

On the condensation of nitrogen in Volta's eudiometer.

When hidrogen gas is burned slowly, rapidly, or instantaneously in a mixture of oxigen and nitrogen, the last [illegible] gas is in part condensed; combining, according to the proportions of the mixture, either with the oxigen alone, or with the oxigen and hidrogen. These effects are sufficiently great in some circumstances, to occasion considerable errours in eudiometrical experiments by Volta's process. This will appear from the following instances, which have been repeated a great many times.

Two hundred parts of hidrogen gas procured by means of zinc, and 200 parts of oxigen, in which the hidrosulphuret of potash indicated 5·75 of nitrogen, left, after their detonation and the action of potash, a residuum equal to 102·5 parts. This result indicates at least 1·7 of nitrogen in the 200 of hidrogen *.

Two hundred parts of the same oxigen gas, and 200 of the same hidrogen, were burned with 200 parts of nitrogen extracted from common air. The residuum of the detonation, after the action of the potash, was only 197·5 parts; but according to the preceding experiment it ought to have been 202·5, if the nitrogen added had not sensibly affected the result.

To judge of the quantity of nitrogen condensed, I treated these 197·5 parts with potash, and with hidrosulphuret, by which they were reduced to 105·45. Now the quantity of nitrogen introduced in this experiment was $100 + 5{\cdot}75 + 1{\cdot}7 = 107{\cdot}45$. The combustion therefore condensed $107{\cdot}45 - 105{\cdot}45 = 2$ parts of nitrogen. This result was the mean of a great number of observations.

In the preceding experiment, where 200 parts of hidrogen, containing 1·7 of nitrogen, were burned with 200 of oxigen, in which 5·75 of nitrogen were included, the latter

* I admit, with Gay-Lussac, that 200 parts of hidrogen condense 100 of oxigen. But I must observe, that the formation of carbonic acid gas, which accompanies the combustion of the [illegible] may modify this assertion in a very slight degree, [illegible] proved to less than a thousandth part of the volume of the hidrogen.

was condensed, but much less than in the present. For the 102·5 parts of gas remaining after the detonation were reduced by potash and hidrosulphuret to 6·84 parts, which indicate a condensation of 0·6 of nitrogen.

Hence it follows, that the errours of Volta's endiometer are not equal with all mixtures, and that the indications of this instrument are more accurate, in proportion as the gas analysed contains less nitrogen.

With atmospheric air the errours balance each other.

When atmospheric air is analysed by Volta's eudiometer, adding to it an equal bulk of hidrogen according to the usual practice, the mixture, when detonated, experiences about the same degree of condensation, as if the process with hidrosulphuret had been employed. I satisfied myself however by means of nitrous gas, that all the oxigen gas of the atmospheric air had not been destroyed in the combustion: but as some nitrogen was condensed, this compensated for the oxigen not destroyed. In this case the indication of Volta's eudiometer was accurate only in appearance.

Compounds formed in the combustion of hidiogen and oxigen contaminated with nitrogen.

Lavoisier, Fourcroy, Seguin, Vauquelin, and Cavendish, found, that water formed by the combustion of hidrogen and oxigen mingled with nitrogen sometimes contained nitric or nitrous acid, and in other cases appeared perfectly pure. The experiments of the French chemists indicate, that a slow combustion is the only mean of preventing the developement of the acid. Cavendish however had said, that when the combination of the gasses was effected by detonation, or a rapid combustion, pure water was obtained free from acidity, when the gas, according to his expression, was phlogisticated after the combustion: and, on the contrary, that nitric acid was formed, when the oxigen gas predominated in the combination. I have found, that the observation of Cavendish was accurate in this respect, that the water is acid, when there is an excess of oxigen gas; but that it does not redden blue colours, when there is an excess of hidrogen after the rapid combustion. At the same time however I have observed, that the purity of the water is apparent only, and that the want of acidity is not owing to the absence of nitric acid, but to its neutralization by ammonia, which is always formed with the nitric acid, either by rapid

Nitric acid always,

but in certain cases neutralized by ammonia.

[illegible] combustion, when the hidrogen gas predominates. [illegible] a globe previously exhausted of air I introduced a mixture of two parts of pure hidrogen gas and one of oxigen contaminated with five per cent of nitrogen. I detonated these by means of the electric spark; and then supplied the place of the gasses destroyed by a similar mixture. Having repeated these operations several times, till the gasses would no longer inflame, in consequence of the superabundance of hidrogen and nitrogen, which gradually accumulated in the globe, I obtained 3·25 gram. [50·2 grs.] of water free from acidity. But this water left by spontaneous evaporation about 13 thousandths of a gramme [0·2 of a gr.] of crystallized nitrate of ammonia; which, being triturated with potash, gave out an evident smell of ammonia, and formed very decided nitrate of potash.

Nitrate of ammonia obtained from various inflammable gasses.

I burned very slowly in atmospheric air a stream of hidrogen gas issuing from a gazometer under the mouth of a glass jar, and collected the water that fell from it after condensation on the sides of the vessel. This yielded nitrate of ammonia by spontaneous evaporation, but in less proportion than in the preceding process, which was conducted in a close vessel. In like manner I obtained the same salt by combustion of all the oxicarburetted hidrogen gasses I tried. The formation of ammonia in these combustions is a fact not hitherto observed.

(To be concluded in our next.)

II.

Description of a Clock Escapement: by Mr. George Prior, *jun. of Otley, in Yorkshire.**

SIR,

Escapement invented.

SINCE I had the pleasure of seeing you, when my father and I were in London, I have invented and made the

* Trans. of Soc. of Arts, vol. XXVII, p. 200. The silver medal and twenty five guineas were voted to Mr. Prior for this invention.

machine

machine now sent to your address; and I will thank you to lay it before the Society of Arts &c. the first opportunity.

This escapement will do for a pendulum of any length, and the friction is so small, that it does not require any oil, for it may be made as little as the tooth of the wheel can sufficiently touch the impelling spring, and yet be properly scaped. Will answer for any pendulum.

It is necessary, that the detent spring, the impelling spring, and the pendulum should all spring from one right line or centre; and that the impelling spring should be so much stronger than the detent spring, as will always be sufficient to unlock the wheel. Requisites.

The machine being wound up, and the pendulum put in motion toward the left side, the impelling spring unlocks the wheel, when a tooth falls against the pallet, where it remains until the pendulum returns; then moving the impelling spring, and the wheel being free, the weight on the axis causes it to advance while it escapes one tooth off the end of the pallet or spring, and another tooth is locked against the detent spring, as before, while the pendulum returns, and the impelling spring again unlocks the detent; as the impelling spring moves towards the left, the resistance it meets with by the elasticity of the detent spring in unlocking is returned to it, so that there is no more power lost than what was caused by the pressure of the wheel against the detent spring; by which means it is freed from the loss of the maintaining power, which is evident in all detached escapements I have seen, by the pendulum or crutch, &c., touching a spring or lifting a lever to regain the position for unlocking the wheel. Its mode of action.

I am, Sir,

Your most obedient humble servant,

GEORGE PRIOR, Jun.

P. S. The screw in the pendulum rod is to adjust the machine when fixed up.

Description of Mr. Prior's Escapement. Plate V, Fig. 1, 2.

Fig. 1 is a side, and fig. 2 a back view of it, which is supposed to be taken from behind the clock; *a* represents the axis Description of the escapement.

axis of the swing wheel, or last wheel of the train of the clock: *b d* is the swing wheel fixed upon it, having 30 serrated teeth, it is turned round in the direction from *b* to *d*, by the maintaing power of the clock, (in the model this power is supplied by the descent of a small weight attached to the end of the small line, which is coiled round the barrel *f*, on the axis of the swing wheel); *g* is a spring detent, which locks against one of the teeth of the swing wheel, and this prevents its running down, by the action of the maintaining power; *h* is another spring detent, which is called the impelling spring, when left at liberty, it unlocks the former by pushing against the end of the small arch, fig. 2, *e*, fastened to the detent *g*, and thus removing the end of the detent which obstructed the wheel's motion; *l* is the rod of the pendulum suspended by a cock screwed to the back plate of the clock; a small piece of brass *k*, fig. 1, projects at right angles from the impelling spring *h*, so as to intercept the pendulum rod in its vibration, and at this place a small screw is put through the pendulum rod *l*, the point of which moves the impelling spring back; a small pin is fixed to the frame in a line between the point of suspension of the pendulum, and in the centre of the swing wheel, against which the impelling spring stops when at liberty.

The action. Supposing the pendulum to be vibrating backwards and forwards, and the wheel locked as in the figure, the pendulum swinging from *m* to *n*, fig. 2, the impelling spring *h* follows by its elasticity, until the pendulum *l* arrives at the perpendicular: at this period the impelling spring comes to rest against the end of the arc *e*, which it pushes back, so as to release the tooth of the wheel from the detent spring *g*; the wheel now moves round a very small space before it meets the end of the impelling spring *h*, and is stopped thereby, in the mean time the pendulum continues its motion the extent of its vibration towards *n*, when it returns, and arriving at the perpendicular, it meets the impelling spring *h*, and carries it along with it, until the tooth of the wheel which rests against it, escapes from the end of it, and another tooth of the wheel comes to rest against the spring detent

detent g. The succeeding vibration of the pendulum repeats the same operation.

III.

An Eye Bath, to clear the Eye from extraneous Matters, and to assist the Sight: by Mr. JOHN DUCKETT ROSS, *No. 55, Princes Street, Leicester square.**

SIR.

BEING employed in the jewellery business, I have frequently suffered greatly from extraneous substances getting into my eyes, at different times, while I have been at work; and I have witnessed many accidents of a similar kind, which have happened to enamel-grinders, turners in metal, jewellers, lapidaries, and other artificers. These circumstances led me to attempt some means, which would relieve such misfortunes, and also strengthen my own eyes, which were naturally weak. I have now succeeded in inventing an eye bath, possessing all these advantages; which I beg leave to lay before the Society of Arts &c., and hope it will be patronized by them.

Eye liable to accidents in certain employments.

Eye bath.

The machine I have sent forms an elegant ornament for a lady's or gentleman's dressing-room, and has been found very serviceable in use, of which I will furnish the Society with certificates.

I remain, Sir,

Your most humble and obedient servant,

JOHN DUCKETT ROSS.

SIR,

I thought it might not be improper to acquaint you, that Mr. Carpue has, according to a promise he made me, this day honoured my invention, by expressing his fullest satisfaction of it before the Medical Board; and that surgeon-general Keate, and Mr. Gillham ,late chief surgeon to the

Recommendations of it.

* Trans. of the Soc. of Arts, vol. xxvii, p. 203. Fifteen guineas were voted to Mr Ross for this invention.

Coldstream regiment, have, in consequence, favoured me with their orders: and I am farther assured, that there is great hopes of my invention being generally adopted for the use of the army and navy.

I am, Sir,
Your most obedient humble servant,
JOHN DUCKETT ROSS,

Certificates were received from several other persons, stating, that they considered Mr. Ross's invention for the eyes calculated to produce many excellent advantages to the public, and likely to become extensively useful.

Description of Mr. Ross's Eye Bath. Plate V, Fig. 3, 4, 5, and 6.

The eye bath described.

Fig. 4, Plate V, is a perspective view of the eye bath, which is preserved in the Society's Repository. This apparatus is supported on a pedestal or tripod. The bath part is represented on a larger scale in section, fig. 5, where *a b* represents a glass vessel, which has a neck at the lower end, and an aperture at its vertex, as is shown in the plan, fig. 6, to fit the eye. The neck is cemented into a brass tube *c c*, which is supported, by being screwed into an ornamental piece of brass work at the top of the pedestal. This tube encloses a common pewter syringe, the end of which is cemented into the neck of the glass vessel, as the section sufficiently explains. The handle *e* of the syringe has a piece of brass screwed to it, which slides up and down, between two pieces of brass at *h*, in the pedestal, and a glass dish *i* is fixed below the frame, to receive any water which may be spilled by accident. When the instrument is used, the glass vessel is to be partly filled with water, (or any other liquor with which the eye is to be syringed,) so as to cover the orifice of the syringe; the patient then places his eye over the aperture in the glass vessel *a b*, and suddenly lifts up the brass slider at *h*, to which the handle of the syringe is fixed, so as to force the liquor contained in the syringe through that in the glass vessel into the eye; the liquor which covers the point of the syringe takes off the force with which the liquor would be thrown into the eye, so as to render the operation not in the least painful.

Its operation gives no pain.

A more

A more commodious, though less elegant form of the same apparatus is shown in fig. 3. The syringe *a* is here placed horizontally, its point being turned up. It is soldered into a vessel of japanned tin, and the glass *e*, which is here globular, is laid loose on the japanned vessel *d*, which contains the liquor that surrounds the point of the syringe. This apparatus, fig. 3, is intended to be placed on a table when used.

A more commodious but less elegant form.

IV.

On Telegraphic Communications, in a Letter from RICHARD LOVELL EDGEWORTH, *Esq. M. R. I. A.*

To Mr. NICHOLSON.

SIR,

IN the second volume of your quarto Journal, you have given a summary of an essay on telegraphs, which I had published in the Transactions of the Royal Irish Academy. The essay concluded in these words, page 324:—"The thing itself must sooner or later prevail, for utility convinces and governs mankind; and however inattention or timidity may for a time impede its progress, I will venture to predict, that it will at some future period be generally practised, not only in these islands, but that it will become a means of communication between the most distant parts of the world, wherever arts and sciences have civilized mankind."

Essay on telegraphs in the Irish Trans.

Telegraphic communications must become prevalent.

Since that time I have seen various inventions similar to each other for the purposes of telegraphic communication, and in particular I have noticed one in your last number, called a homograph. Now I actually practised such a contrivance twelve or thirteen years ago, and I had during last month drawn up a detailed view of the scheme for the purpose of recommending it, not for the navy, but for the army. I do not, however, by any means, wish to derogate from the merit or the claims of the gallant officer, who serves his country with so much energy both of body and mind, but

Various inventions for the purpose.

to give him the advantage of what much experience has taught me. Lieut. Spratt points out the convenience of having some dark object behind the man who makes his telegraphic signals. This observation shows me, that he has limited his project to the communication of intelligence to a single post from the place whence it is dispatched. Now there are no bounds, except the ocean, to the distances between which intelligence may be conveyed by men alone, without the intervention of any apparatus but a telescope.

Communications need not be limited to a single person, but may be conveyed over any extent of land.

For this purpose the signalman must be *legible* both behind and before.

Repetition best shows that the signal is understood.

In Mr. Spratt's arrangement there is a signal to denote, that the operator is understood; the best means of ascertaining this, is a repetition of the signal by the person that receives it: and this, through a long line of communication, gives no delay except at the second station.

I shall not encumber your Journal with any particular detail of the arrangements, which I had made for this scheme; they may be varied ad infinitum.

Nobody but a fastidious critic will find fault with the gallant lieutenant for calling his telegraph a homograph; it should however be called an andrograph, or homoscribe, or by some English name.

Generals should not neglect the resources of art.

I take this opportunity of observing, that if generals of large armies would employ the resources of art as well as those of mere physical force, they would save much time and blood; they would avoid much disappointment and disgrace; and whether they ultimately failed or succeeded, they would have the satisfaction of knowing, that they had neglected no reasonable means of ensuring success.

I am, Sir,

Your obedient servant,

RICHARD LOVELL EDGEWORTH.

New

V.

New Theory of the Diurnal Motion of the Earth round its Axis. In a Letter from Professor Wood.

To Mr. NICHOLSON.

Richmond Academy, State of Virginia,
SIR, *4th Feb.* 1810.

THE theory, of which this circular letter gives you a very brief account, is published in English; but being desirous of having the opinion of several of the mathematicians on the continent of Europe, as to the correctness of the principle, I was induced to write the circular in the French language. If it appear to you to merit notice, some account of it in your Journal will much oblige,

Sir,

Your obedient servant,

JOHN WOOD.

SIR,

New theory of the rotary motion of the Earth.

I have just published in this city a work entitled, "A new Theory of the Diurnal Rotation of the Earth, demonstrated upon mathematical Principles, from the Properties of the Cycloid and Epicycloid: with an Application of this Theory to the Explanation of the various Phenomena of the Winds, Tides, and those stony and metallic Concretions, which have fallen from Heaven upon the Surface of the Earth."

Proposed to mathematicians in general.

This theory I have made a point of communicating to all those whom I consider eminently distinguished for their knowledge of the mathematics. It is for them to be my judges, and to decide on the solidity of the principle, which forms the basis of my work. In short, I wish to know their opinions for or against me. Allow me therefore, sir, to impart to you as concisely as possible the circumstance, which gave rise to my theory, and to the fundamental principles it includes.

In

Bet on the motion of a wheel.

In the beginning of last summer two gentlemen in Richmond laid a considerable wager on the following question: "Do the top and bottom of a cart or carriage wheel, when in motion, move with equal or unequal velocities?"

The top has greater velocity than the bottom.

When the question was first proposed to me, I certainly was of opinion, that there could be no difference in the velocity of any point in the same circumference of the wheel: but upon reflecting, that every point of a carriage wheel moving along a right line in a horizontal plane describes a cycloid, a leading property of which curve is for the generating point to describe unequal arcs in equal times, I was convinced of my errour; and perceived, that any point in the upper semicircle of the wheel must move with greater velocity than the corresponding and opposite point in the under semicircle.

This applicable to the motion of the Earth.

This truth immediately suggested the application of the same principle to the motion of the Earth; for it is evident, that the motion of any point on the Earth's surface, with the exception of the two poles, being compounded of two motions, a rotary motion round the axis of the Earth, and a progressive motion along the plane of the ecliptic, will also describe a curve of the cycloidal, or rather epicycloidal species, possessing a similar property with the common cycloid generated by a carriage wheel.

Important consequences deducible from this,

The cycloidal motion on the points of the Earth's surface being once established, several important consequences obviously present themselves. For it is manifest, if every point in the same parallel of latitude vary its velocity in revolving round the axis of the Earth, the centrifugal force of that point must also vary; that is, when the velocity of the point is greatest, the centrifugal force will also be greatest; and on the contrary, when the velocity of the point is least, the centrifugal force will also be least. This variation in the centrifugal force of every point on the Earth's surface, during a diurnal rotation, necessarily affecting the fluids which encompass the Earth; it appeared to me, that the phenomena of the tides, the trade winds, and several other phenomena in nature, might thereby be explained. On this subject I published, in the month of May, several essays in the Inquirer and Virginia Argus of Richmond. These

as the centrifugal force must vary.

This accounts for the tides, trade-winds, &c.

essays

essays having received the approbation of many gentlemen well acquainted with the mathematical and physical sciences, I was induced to enter into a farther investigation of the effects arising from this principle of cycloidal motion. The result of this investigation is contained in the work above-mentioned: and to give you a general idea of the principles on which my theory is founded, I shall recite those propositions of my work, which appear to me most important.

Prop. 8, Book I. If the semicycloid A a *A* be described Prop. 1.
by the point A of the circle A D B E, Pl. V, fig. 7, revolving from B to A; and the semicycloid B b *B* be at the same time described by the opposite point of the generating circle; then the cycloidal arcs A a and B b, described in equal times, will be to each other as the chord A F of the circular arc A F to the difference between the diameter A B of the generating circle and the chord *B* H of the supplement of the same arc.

Prop. 9. The velocity of the point A at a is to the velo- Prop. 2.
city of B at b, as $\sqrt{BL}$ to $\sqrt{AL}$.

Prop. 12. If the circle A D B E move from B to *A* with Prop. 3.
two uniform motions, a rotatory motion and a progressive motion, so that the two opposite points A and B describe two semicycloidal curves A a *A* and B b *B*, and the progressive velocity be to the rotary velocity as n to 1: then the velocity of the point A in any place, a, will be to the velocity of the point B in any place, b, as

$$\sqrt{1+\frac{n \times AC + AC - AL^2}{2\,AC \times AL - AL^2}}$$

$$\text{is to } \sqrt{1+\frac{n \times AC + AC - BL^2}{2\,AC + BL - BL^2}}$$

In order to apply this formula to the motion of the Earth, I suppose it to move along the chord of an elliptical arc every twenty-four hours, instead of the arc itself, over which it really moves: and having shown, that the velocity of any part on the surface of the globe, except at the poles, is greatest at noon, and least at midnight, I prove, that the velocities

velocities of any two opposite points of the equator are to each other, as

$$\sqrt{1+\frac{64\cdot4-AL^2}{2AL-AL^2}} \text{ is to } \sqrt{1+\frac{64\cdot4-BM^2}{2BM-BM^2}}.$$

Difference of velocities at the equator.

From this formula I have deduced, that the velocity of any point of the equator at noon is to the velocity of the same point at midnight, as 3690 to 3502, or as 1·053 to 1 *. In like manner I have found, that the velocity at 1 o'clock P. M. is to the velocity at 1 A. M. as 248·9 to 241·4, or as 1·032 to 1.

Effects on gravitation,

I afterward calculate the effects, which this difference in the velocities of two opposite points in the same parallel of latitude would produce on the force of gravity at the surface of the Earth, and I find, that, under the equator, bodies lose at noon a 9375th of their weight. I then demonstrate, that the effect, which this difference in the gravity of bodies produces upon the matter and fluids on the surface of the globe is 306 times greater than † the effect produced by the attraction of the moon, and 1372 times greater than any effect produced by the sun.

and consequently on the sea, &c.

Application to phenomena.

This principle I have employed to explain the tides, the trade winds, and the phenomena of falling stones ‡. My theory necessarily leads into researches of too great length, to find a place in this brief analysis: I confine myself therefore, Sir, to request your serious examination of the principle of cycloidal motion, which I ascribe to every point of the surface of the globe, and the effects which this motion must produce on bodies at its surface. Your ideas on this subject will highly oblige me.

I have the honour to be,

Sir,

Your very obedient, and very humble servant,

JOHN WOOD.

* In the work itself, which is now before me, there is a list of errata, which makes these numbers as 3718 to 3002, or as 1·034 to 1. C.

† It should be "306 times as great as." C.

Meteoric stones.

‡ Prof. Wood supposes the stones, that fall from the atmosphere, to be projected into it from volcanoes: and that, as the point from which they are thrown has its rotary velocity increased or diminished, while the stones retain that impressed on them at the time of their projection, they must consequently reach the Earth at a greater or less distance east or west of the volcano. C.

Method

VI.

Method of securing the Beams of Ships, without wooden Knees made of one Piece: by Mr. George Williams, *Master Carver at his Majesty's Dock Yard, Chatham*.*

SIR,

I Submit to you, for the inspection of the Society, the following particulars of my invention for the better securing of the beams of ships of war, East and West India ships, and all others where strength, dispatch, room, and cheapness are required. In this method less iron in weight, and fewer bolts are necessary, than in the iron knees before in use; there is also less strain upon the bolts, as the block underneath is morticed both into the beam and side of the ship, as well as bolted.

Advantages of this method of securing a ship's beams.

Upon this plan the work is all done under the hand, which is executed much quicker than in the former plan, where the work is all done over hand, and where great nicety is requisite in making the bolt-holes which pass through both the iron stays. In my method much more room is also gained between decks for stowage and working the guns, and even a porthole may be made under the beam itself.

I calculate the saving in a 74-gun ship or East Indiaman to be as follows, viz.

Saving.

	Ton.	Cwt.	qrs.	lb.		s.	d.		£.	s.	d.
Copper bolts,	2	1	3	9	at	1	4	*per lb.* ··	305	8	0
Iron,	2	12				56		*per cwt.* ··	145	12	0
Three men and one boy's time for a month ····									45	17	6
Timber ································									50	0	0
									£546	17	6

* Trans. of the Society of Arts, vol. XXVII, p. 142. The silver medal was voted to Mr. Williams for this invention.

The

The models I have sent will, I trust, clearly explain to the Committee every circumstance, which will be thought necessary.

I am, Sir,
Your respectful humble servant,
GEORGE WILLIAMS.

Description of Mr. Williams's Method of connecting the Beams of Ship's Decks to their Sides, Plate VI, Fig. 1, 2, and 3.

Explanation of the plate.

Fig. 1 is a horizontal plan of a portion of a ship's side; the planks of the deck being removed, to show the ends of two of the beams A A, which extend across the vessel. B B is the outside planking of the ship; C C the sections of the timbers or ribs; and to these the beams A A are fastened by beaten iron triangular braces *a a*, similar in form to the Roman capital letter A. These are let into the beams, and attached there at the angle by three bolts going through them. Fig. 2 is a front view of only one beam, where the spectator is supposed to be looking towards the ship's side; and fig. 3 is part of a cross section of the vessel's side; the same letters are used as in the other figures. By inspecting these, it will be seen, that the ends of the brace *a a* are turned up and bolted to the timbers of the ship's side by two bolts passing through each end, and through the timbers and the outside planks; by which means the beams are secured from lateral motion: and to brace them in a vertical direction the wooden block H, fig. 3, is fitted in beneath them, and two iron straps bolted on them; one end of each of these straps is attached to the deck beams by the same bolts as the upper brace *a a;* the other ends are bolted against the inside planking, and an oblique bolt *h*, fig. 3, passes through the middle of each strap and the ship's side; *l l l*, fig. 1, are the small intermediate beams, answering to the joists of a floor, to which the planks of the deck are spiked down; *m*, fig. 1, represents one of the planks, and the dotted lines show the joints of the others. In fig. 3, these planks are shown, and the other beams to make all sound and firm, which were removed in the other figure to show the braces.

VII.

Method to prevent the Accidents which frequently happen from the Linchpins of Carriages breaking or coming out: by Mr. J. VARTY, of Liverpool, Coachmaker.*

SIR,

HEREWITH you will receive a model of an axle-tree for public machines, intended to prevent the wheel from coming off, if the linchpin should break, and thereby prevent many dangerous consequences. When the idea first suggested itself to me, I put it in practice in a stage coach, which has since run from Liverpool to Litchfield, a distance of eighty-four miles, six days per week, for the last six months. During that time several instances have occurred in which the linchpins have broke or come out, but owing to this contrivance no accident has happened therefrom. We almost daily hear of stage coaches being upset, which more frequently arises from linchpins breaking than from any other cause.

Contrivance to prevent accidents from the linchpin of a wheel breaking or slipping out.

In offering this model to the Society of Arts &c. for their inspection, I anticipate the pleasure of their sanction, as I can furnish satisfactory vouchers of its proved utility.

I am, Sir,

Yours, respectfully,

J. VARTY.

Description of Mr. Varty's Linchpin.

Mr. Varty's contrivance is shown in fig. 4, and 5, Plate VI. Fig. 4 is a section of the nave of a carriage wheel, with the axle-tree A A in it; and fig. 5 is a separate view of the axletree. *a*, fig. 5, is the linchpin detached; it is put through an oblong hole in the axle as usual, but there is likewise an additional linchpin *b*, to make it complete, which is fixed in a recess cut for it in the axle, and turns on a pin (as is shown in the figure) into the hole left by re-

Described.

* Trans. of the Society of Arts, vol. XXVII. p. 145. For this invention the silver medal was voted to Mr. Varty.

moving

moving the linchpin *a*, when the wheel is to be taken off; but if the linchpin *a* should accidentally get out, this additional pin *b* would effectually keep the wheel on, as its hanging position does not at all tend to shut the pin up into the axle, but the contrary. The common linchpin *a* is put in downwards, and its weight may also tend to keep it in, and is secured in the usual way by a strap, the holes for which may be seen in the figures. The whole, when in its place, is shown at fig. 4.

VIII.

An Analysis of several Varieties of British and Foreign Salt (Muriate of Soda), with a view to explain their Fitness for different economical Purposes. By WILLIAM HENRY, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester*.*

SECT. I. *General Observations.*

Object of the author.

IN undertaking the series of experiments, described in the following pages, I had not so much in view the discovery of novelties in science, as the determination, by the careful employment of known processes, and by the improvement of methods of analysis, of a number of facts, the establishment of which (it appeared to me probable) might have an influence on an important branch of national revenue and industry.

British salt generally considered of inferior quality.

An opinion has for some time past existed, and I believe has been pretty general both in this and other countries, to the disadvantage of British salt as a preserver of animal food; and a decided preference has been given to the salt procured from France, Spain, Portugal, and other warm climates, where it is prepared by the spontaneous evaporation of sea water. In conformity with this opinion, large sums of money are annually paid to foreign nations, for the supply of an article, which Great Britain possesses, beyond

* Philosophical Transactions for 1810, Part I.

almost

almost any other country in Europe, the means of drawing from her own internal resources. It becomes, therefore, of much consequence to ascertain, whether this preference of foreign salt be founded on accurate experience, or be merely a matter of prejudice; and, in the former case, whether any chemical difference can be discovered, that may explain the superiority of the one to the other.

The comparative fitness of these varieties of salt for the curing of provision, which has been a subject of much controversy among the parties who are interested, can be decided, it is obvious, in no other way, than by a careful examination of the evidence on both sides. Where evidence, however, is doubtful, and where there exists, as in this case, much contrariety of testimony, it cannot be unfair to yield our belief to that, which best accords with the chemical and physical qualities of the substances in question. Again, if salt of British production should be proved to be really inferior in chemical purity to foreign salt, it would be important to ascertain, as the basis of all attempts toward its improvement, in what, precisely, this inferiority consists. It seemed desirable, also, to examine whether any differences of chemical composition exist among the several varieties of home-made salt, which can explain their variable fitness for economical purposes.

This of importance to be ascertained.

Such were the considerations that, induced me to undertake an inquiry, which has occupied, for several months past, a large share of my leisure and attention. I began the investigation, wholly uninfluenced by any preconceived opinions on the subject; and I had no motive to see the facts in any other than their true light, since I have no personal interest, either directly or remotely, in the decision of the question.

The present examination impartial.

The principal sources of the salt, which is manufactured in this country, are rock salt, brine springs, and seawater. The first material is confined entirely, and the second chiefly, though not wholly, to a particular district of Cheshire. Of the extent and boundaries of this district, the process of manufacture, and other circumstances interesting to the mineralogist as well as to the chemist, an ample and excellent history has been given by Mr. Henry Holland, in the

Sources of British salt.

Agricultural

Agricultural Report of the county of Chester*. From his account I shall extract, in order to render some parts of this memoir more intelligible, a very brief statement of the characteristic differences of the several varieties of salt, which are prepared in Northwich, and its neighbourhood.

Preparation of stoved or lump salt. In making the *stoved* or *lump salt*, the brine is brought to a boiling heat, which, in brine fully saturated, is 226° of Fahrenheit. This temperature is continued during the whole process; and as the evaporation proceeds, small flakey crystals continue to form themselves, and to fall to the bottom of the boiler. At the end of from eight to twelve hours, the greatest part of the water of solution is found to be evaporated; so much only being left, as barely to cover the salt and the bottom of the pan. The salt is then removed into conical wicker baskets, termed *barrows*; and, after being well drained, is dried in stoves, where it sustains a loss of about one seventh of its weight.

Separation of its impurities. On the first application of heat to the brine, a quantity of carbonate of lime, and sometimes a little oxide of iron, both of which had been held in solution by an excess of carbonic acid, are separated; and are either removed by skimming, or are allowed to subside to the bottom of the pan, along with the salt first formed, and with some sulphate of lime; and are afterward raked out. These two operations are called *clearing* the pan. Some brines scarcely require them at all, and others only occasionally. The whole of the impurities, however, are not thus removed; for a part, subsiding to the bottom, forms a solid incrustation, termed by the workmen *pan-scale*. The portion of this, which is lowest, acquires so much induration and adhesion to the pan, that it is necessary to remove it, once every three or four weeks, by heavy blows with a pick-axe. These sediments are formed, also, in making the other varieties of salt.

Common salt. In preparing *common salt*, the brine is first raised to a boiling heat, with the double view of bringing it as quickly as possible to the point of saturation, and of clearing it from its earthy contents. The fires are then slackened, and the

* Published in 1808.

evaporation

evaporation is carried on for 24 hours, with the brine heated to 160° or 170° Fahrenheit. The salt, thus formed, is in quadrangular pyramids or hoppers, which are close and hard in their texture. The remainder of the process is similar to that of making stoved salt, except that, after being drained, it is carried immediately to the store house, and not afterward exposed to heat, an operation confined to the stoved salt.

Large grained flakey salt.

The *large grained flakey salt* is made with an evaporation conducted at the heat of 130 or 140 degrees. The salt thus formed is somewhat harder than common salt, and approaches more nearly to the cubic shape of the crystals of muriate of soda.

Large grained or fishery salt.

Large grained or *fishery salt*, is prepared from brine heated only to 100° or 110° Fahrenheit. No perceptible agitation, therefore, is produced in the brine, and the slowness of the process, which lasts from 7 or 8 to 10 days, allows the muriate of soda to form in large, and nearly cubical crystals, seldom however quite perfect in their shape*.

Uses of the different kinds.

For ordinary domestic uses, stoved salt is perfectly sufficient. Common salt is adapted to the *striking* and salting of provision, which is not intended for sea voyages or warm climates. For the latter purposes, the large grained or fishery salt is peculiarly fitted.

Scotch salt from seawater.

On the eastern and western coasts of Scotland, and especially on the shores of the Frith of Forth, large quantities of salt are made by the evaporation of seawater. In consequence of the cheapness of fuel, the process is carried on, from first to last, by artificial heat, at a temperature, I believe, equal or nearly so to the boiling point, and varying, therefore, according to the concentration of the brine. The kind of salt, chiefly formed in Scotland, approaches most nearly to the character of stoved salt. In some places a salt is prepared, termed *Sunday salt*; so called, in consequence of the fires being slackened between Saturday and Monday, which increases considerably the size of the crystals.

Sunday salt.

Dr. Thomson.

I am indebted to Dr. Thomson of Edinburgh (who gave

* Cheshire Report, p 53, &c.

me his assistance with great zeal and alacrity) for an opportunity of examining upwards of twenty specimens of Scotch salt, prepared by different manufacturers. That distinguished chemist, it appears from a letter which he addressed to me on the subject, was some time ago engaged in experiments on Cheshire salt. The particulars he has lost; and he retains only a general recollection of the facts, which confirms, I am happy to state, the accuracy of the results obtained by my own experiments.

Lymington salt from sea-water.

At Lymington, in Hampshire, advantage is taken of the greater heat of the climate, to concentrate the seawater by spontaneous evaporation to about one sixth its bulk, before admitting it into the boilers. One kind of salt is chiefly prepared there, which most nearly resembles in grain the stoved salt of Cheshire. The process varies a little in some respects, from that which has been already described. The salt is not fished (as it is termed) out of the boiler, and drained in baskets; but the water is entirely evaporated, and the whole mass of salt taken out at once, every eight hours, and removed into troughs with holes in the bottom.

Bittern.

Through these it drains into pits made under ground, which receive the liquor called *bittern* or *bitter liquor*. Under the troughs, and in a line with the holes, are fixed upright stakes, on which a portion of salt, that would otherwise have escaped, crystallizes and forms, in the course of ten or twelve days, on each stake, a mass of sixty or eighty pounds.

Salt cats.

These lumps are called *salt cats*. They bear the proportion to the common salt, made from the same brine, of 1 tun to 100.

Preparation of sulphate of magnesia.

From the mother brine, or bitter liquor, which has drained into the pits, the sulphate of magnesia is made during the winter season, when the manufacture of salt is suspended, in consequence of the want of the temperature, required for the spontaneous evaporation of the sea-water. The process is a very simple one*. The bitter liquor from the

* I am indebted for an account of this process, as well as of the method of making common salt at Lymington, to the liberal communication of Charles St. Barbe, Esq., of that place. Though not strictly connected

the pits is boiled for some hours in the pans, which are used in summer to prepare common salt; and the impurities, which rise to the surface, are removed by skimming. During the evaporation, a portion of common salt separates; and this, as it is too impure for use, is reserved for the purpose of concentrating the brine in summer. The evaporated bitter liquor is then removed into wooden coolers 8 feet long, 5 feet wide, and 1 foot deep. In these it remains twenty-four hours, during which time, if the weather prove clear and cold, the sulphate of magnesia, or Epsom salt, crystallizes at the bottom of the coolers, in quantity equal to about one eighth of the boiled liquor. The uncrystallizable fluid is then let off through plugholes at the bottom of the coolers; and the Epsom salt, after being drained in baskets, is deposited in the store-house. This is termed *single Epsom* salts; and after solution, and a second crystallization, it acquires the name of *double Epsom* salts. Four or five tuns of sulphate of magnesia are produced from a quantity of brine, which has yielded 100 tuns of common, and 1 tun of cat salt.

Seawater saturated with rock salt.

On the banks of the Mersey, near its junction with the Irish Channel, the water of that river before evaporation is brought to the state of a saturated brine, by the addition of rock salt. The advantage of this method of proceeding will be obvious, when it is stated, that 100 tuns of this brine yield at least 23 tuns of common salt, whereas from the same quantity of seawater, with an equal expenditure of fuel, only 2 tuns 17 cwt. of salt can be produced*.

Rock salt used for provision.

Within the few past years, an attempt has been made to apply rock salt itself to the packing of provision. For this

connected with the subject, I give his description of the mode of making Epsom salt, because no correct statement of the process has, I believe, been hitherto published. The analysis of seawater, indeed, by a justly distinguished chemist (Bergman), excludes, erroneously, the sulphate of magnesia from its composition; and his results have led to the opinion, that, to manufacture this salt on the large scale, requires the addition either of sulphuric acid, or of some sulphate to the *bitter liquor*. (See Aikin's Chemical Dictionary, II, 388.)

Errour of Bergman.

* See the Earl of Dundonald's "Thoughts on the Manufacture and Trade of Salt." London, 1785.

purpose

purpose it is crushed to the proper size between iron rollers. The trials which have been made, I am informed, are but few, and the results hitherto are not perfectly known.

Bay salt.

The *bay salt* imported from foreign countries is well known to be prepared by the spontaneous evaporation of seawater, which, for this purpose, is confined in shallow pits, and exposed to the full influence of the sun and air. I have no addition to make to the accounts of its manufacture, which have already been given by various writers*.

Results of the examination, and analyses.

As the results of the investigation, which forms the subject of this memoir, may be acceptable to many persons, who can scarcely be expected to take an interest in a long detail of analytical processes, I shall present in the following section a general view of the experiments, and of the conclusions that may be deduced from them. In the last place, in order that other chemists may be enabled to repeat the analyses under similar circumstances, I shall describe minutely the methods that were adopted, some of which are new, and others reduced to greater precision. If however, in the future progress of science, it should appear, that any of these processes are imperfect, it may still be admitted, that, for all useful purposes, they afford a fair *comparison* of the composition of the several varieties of culinary salt; since the sources of fallacy, that may hereafter be discovered, must have been the same in every case, and have produced in each an errour of nearly the same amount.

SECT. II. *General Statement of the results of the Experiments, and Conclusions that may be deduced from them.*

Different salts compared.

A comparison of the component parts of British and foreign salts, and of different varieties of British salt with each other, will best be made by an examination of the following table, which comprehends the results of the analysis of equal weights of each variety.

* Encyclop. Méthod. Art. Salins. (Des Marais Salans). [illegible] Dictionary of Chemistry, II, 224. Watson's Chemistry, Vol. [illegible]. It is necessary to remark, that a great proportion of what is sold in London as bay salt is Cheshire large grained fishery salt.

1000

Their component parts.

1000 parts by weight consist of

	Kind of salt.	Insol. mat-ter	Muriate of lime	Muriate of magnesia	Total earthy muriates	Sulph. of lime.	Sulph of magnesia	Total sul-phates.	Total impuri-ties.	Pure muriate of soda
Foreign bay salt	St. Ube's	9	a trace	3	3	23¼	4½	28	40	960
	St. Martin's	12	ditto	3½	3½	19	6	25	40½	959½
	Oleron	10	ditto	2	2	19½	4¾	23¾	35¾	964¼
British salt from salt water	Scotch (common)	4	——	28 or +	28 or +	15	17¼	32½	64½	935½
	Scotch (Sunday)	1	——	11½	11½	12	4½	16½	29	971
	Lymington (common)	2	——	11	11 or +	15	35	50	63	937
	Ditto (cat)	1	——	5	5	1	5	6	12	988
Cheshire salt.	Crushed rock	10	0·1/10	0·3/10	0·¼	6½	—	6½	16¾	983¼
	Fishery	1	0·¼	0·¾	1	11½	—	11¼	13	986¾
	Common	1	0·¼	0·¼	1	14½	—	14½	16½	983½
	Stoved	1	0·¼	0·¼	1	15½	—	15½	17½	982½

I. The

Proportion of impurities.

I. The *total amount of impurities*, and the *quantity of real muriate of soda*, contained in each variety of common salt, may be learned by inspecting the last two columns of the table. From these it appears, that the foreign bay salt is purer, generally speaking, than salt which is prepared by the rapid evaporation of seawater; but that it is contaminated with about three times the amount of impurities discoverable in an equal weight of the Cheshire *large-grained* salt, and with more than twice those that are found in the *stoved* and *common* salt of the same district.

Insoluble matter.

II. The *insoluble matter* in the foreign salt, after the action of boiling water, appears to be chiefly argillaceous earth coloured by oxide of iron; and is probably derived in part from the pits, in which the seawater is submitted to evaporation. We may, perhaps, assign the same origin to the very minute portion of muriate of lime, which is not found in the salt prepared by evaporating seawater in metallic vessels, or even in the mother liquor, or uncrystallizable residue. In seasalt prepared by rapid evaporation, the insoluble portion is a mixture of carbonate of lime with carbonate of magnesia, and a fine silicious sand; and in the salt prepared from Cheshire brine, it is almost entirely carbonate of lime. The insoluble part of the less pure pieces of rock salt is chiefly a marly earth, with some sulphate of lime. The quantity of this impurity, as it is stated in the table, is considerably below the average, which in my experiments has varied from 10 to 45 parts in 1000. Some estimate of its general proportion, when ascertained on a larger scale, may be formed from the fact, that government, in levying the duties, allows 65lb. to the bushel of rock salt, instead of 56lb., the usual weight of a bushel of salt.

Earthy muriates.

III. The *earthy muriates*, and especially that with base of magnesia, abound most in salt which is prepared by the rapid evaporation of seawater. Now since common salt, in all its forms, contains, as will afterward appear, very little water of crystallization, it is probable, that the muriate of magnesia, discovered by the analysis of seasalt, is derived entirely from that portion of the mother liquor, which adheres to the salt after being drained, and which amounts to about one seventh of its weight. The larger the size of the grain, the

the less is the quantity of this solution, which the salt holds suspended; and hence the salt prepared at a lower degree of heat, being in larger crystals, is less debased by the magnesian muriate, than the salt formed at a boiling temperature. It is probable also, that, when the salt is drawn at intervals from the boiler, the proportion of the earthy muriate will vary with the period of the evaporation, at which it is removed. For it may readily be conceived, that as the proportion of the earthy muriates in any brine is increased by the separation of muriate of soda, the greater will be the quantity of these muriates, which the crystals of common salt, formed in the midst of the brine, will retain. It follows therefore, that, so far as the earthy muriates only are concerned, salt must diminish in purity as the process of evaporation advances.

Cheshire salt.

In the several varieties of Cheshire salt, the earthy muriates do not exceed one thousandth part of their weight, and they are precisely (or so nearly so, that the difference is not ascertainable) the same in all. This will cease to be matter of surprise, when it is considered, that the salt obtained by evaporating to dryness the whole of a portion of Cheshire brine does not give more than 5 parts of earthy muriates in 1000. In the entire salt of seawater, according to Bergman, the earthy muriates form no less than 213 parts in the same quantity.

[illegible] muriate o[illegible] attracts moisture.

According to the proportion in which the earthy muriates are present in any kind of salt, will be its power of deliquescence, or of attracting moisture from the atmosphere. It is not entirely, however, from the salts with earthy base, that common salt derives this quality; for the most transparent specimens of rock salt, which I find to consist of absolutely pure muriate of soda, attract much moisture from a humid atmosphere.

Earthy sulphates.

IV. The *sulphate of magnesia* and the *sulphate of lime* both enter into the composition of all the varieties of salt prepared from seawater; but the sulphate of lime alone is found in Cheshire salt. The proportion of sulphate of magnesia is greatest in that variety of seasalt, which has been formed by rapid evaporation. In foreign bay salt, its quantity is very insignificant.

From

Sulphate of lime.

From the table it may be seen, that the proportion of sulphate of lime is greater in foreign bay salt, than in any variety of British salt, even than in those which are prepared from seawater with a boiling heat. The only explanation of this fact that occurs to me is, that, during the rapid evaporation of seawater, a considerable part of the calcareous sulphate is precipitated at an early stage of the process, and is partly removed in *clearing* the boiler; a process, which can scarcely be performed during the formation of bay salt in pits, the sides of which are composed of moist clay. The remainder of the selenite, thus precipitated by the rapid evaporation of seawater, enters into the composition of the pan-scale.

Same kinds of salt differ.

In the course of this inquiry, I was induced to repeat the same experiments several times, on various specimens of salt bearing the same designation; and was surprised to find, that the results by no means corresponded. In one instance, for example, fishery salt was found in 1000 parts to contain no less than 16 parts of sulphate of lime; while another specimen, nominally the same, contained only 11¼ parts of selenite in the same quantity; and a third only 5¼. At length it occurred to me, that these differences were probably owing to the circumstance of the salt having been taken from the boiler at different periods of the evaporation. I requested, therefore, to be furnished with specimens of salt, drawn at different stages of the process, from a given portion of brine, evaporated in the same boiler. These were submitted to analysis; and the results are shown in the following table.

	Contained in 1000 parts.	Sulphate of lime.
Common salt drawn from the boiler, 2 hours after the first application of heat		16
Salt drawn 4 hours after do.		11¼
Salt drawn 6 hours after do.		3¼

First made salt contains most sulphate of lime,

Hence it appears, that there was a gradually increasing purity in the salt from sulphate of lime, as the process of evaporation advanced; the greatest part of this earthy compound being deposited at an early stage of the process. Different specimens of the same kind of salt may, therefore, differ in chemical purity as much from each other, as from

from other varieties. But when the impurities, contained in a solution of muriate of soda, are of a different species from those of Cheshire brine, and consist chiefly of the earthy muriates, the order will be reversed, and the purest salt, as I have already suggested, will be that which is first deposited; the contamination with the muriate of lime or of magnesia continuing to increase, as the process advances to a conclusion *. and least earthy muriate.

At an early period of the inquiry it appeared to me probable, that the differences between the several varieties of culinary salt might depend, in some degree, on their containing variable proportions of water of crystallization. It was found, however, by experiment, that the proportion of water in any variety of common salt, after being dried at 212° Fahrenheit, is not much greater or less than that which is contained in any other variety. Pure transparent rock salt, calcined for half an hour in a low red heat (= 4° or 5° of Wedgwood's pyrometer), lost absolutely nothing of its weight. It is remarkable, also, that the pure native salt, if free from adventitious moisture, may be suddenly and strongly heated, with scarcely any of that sound called *decrepitation* †, which is produced by the similar treatment of all the varieties of artificial salt. Even these varieties, Water of crystallization nearly similar in all salt, dried at the same heat.

* I cannot on any other principle explain the considerable differences, as to the proportion of muriate of magnesia, that were discovered in the several varieties of Scotch salt, sent to me by Dr. Thomson. For this reason, in stating the analysis of Scotch salt, I have given, in the table, that result which was most frequently obtained; and have withheld the names of the manufacturers, because the differences were probably in a great measure accidental, and not the result of greater or less skill in the preparation. One specimen of Lymington salt, which I examined, contained fully as much muriate of magnesia as any of the Scotch samples. The *cat salt* of that place, however, contrary to my expectation, proved to possess a very extraordinary degree of purity; a fact of which I satisfied myself by repeated experiments.

† Decrepitation is occasioned by the sudden conversion into vapour of the water contained in salts, when its quantity is insufficient to effect the watery fusion. It is a property peculiar to salts which hold only a very small proportion of water in combination, as muriate of soda, nitrate of lead, and sulphate of potash. Decrepitation;

however,

however, exposed during equal times to a low red heat, do not lose more than from half a grain to three grains in one hundred. This comparison cannot be extended to the salt prepared at a boiling temperature from seawater; because the muriate of magnesia, which these varieties contain, is decomposed at a red heat, and deprived of its acid.

The following table shows the quantity of water contained in several kinds of salt, inferred from the loss which they sustain by ignition during equal times, after being first dried at 212°.

Proportions of this water.

100	parts of large grained fishery salt contain of water	3
100	...... foreign bay salt (St. Martin's)	3
100	...... ditto .... (Oleron)	$2\frac{1}{2}$
100	...... ditto Cheshire common salt	$1\frac{1}{2}$
100	...... ditto .... stoved salt	$0\frac{1}{4}$

The loudness and violence of the decrepitation was, as nearly as could be judged, in the same order, and was most remarkable in the large grained varieties.

Proportions of real muriate given by nitrate of silver.

To determine the proportions of real muriate of soda in those varieties of artificial salt which are nearly free from earthy muriates, I employed also the process of decomposition by nitrate of silver. The following are the quantities of fused *luna cornea*, obtained from 100 grains of each of three varieties dried, previously to solution, at the temperature of 212° Fahrenheit.

100	gr. pure transparent rock salt gave of luna cornea	242
100	.. stoved salt, remarkably pure	239
100	.. fishery salt, ditto	237*

* From 100 grains of pure artificial muriate of soda, previously heated to redness, Dr. Marcet has since informed me, that he obtained 241·6 grains of fused luna cornea. The weights of the precipitates thrown down in my experiments by nitrate of silver are not, I am aware, exactly those which might have been expected from the table of the comparative proportions of water given in the text. Each experiment, however, was twice repeated with every precaution I could adopt, and with the same results. That different kinds of salt give different proportions of luna cornea, is proved also by comparing the experiment of Dr. Marcet with the results of Dr. Black and Klaproth, both of whom found the fused muriate of silver, from 100 parts of common salt, to weigh 235 grains.

V.

The

The proportion of ingredients in the several kinds of muriate of soda (setting apart the impurities) appears, therefore, to be nearly the same in all. And as the very minute quantity of water, discovered by analysis, is not constant in the several varieties, it may be inferred to be rather an accidental than a necessary ingredient; for in the latter case an invariable proportion might be expected, conformably to the important law, establishing a uniformity in the proportions of chemical compounds, which has been explained by Mr. Dalton, and confirmed by Drs. Thomson and Wollaston.

Proportions of ingredients, impurities excepted, nearly the same in all.

What then, it may be inquired, is the cause of those differences, which are acknowledged, on all hands, to exist among the several species of muriate of soda, so far as respects their fitness for economical purposes? If I were to hazard an opinion, on a subject about which there must still be some uncertainty, it would be, that the differences of *chemical composition*, discovered by the preceding train of experiments, in the several varieties of culinary salt, are scarcely sufficient to account for those properties, which are imputed to them on the ground of experience. The *stoved* and *fishery* salt, for example, though differing in a very trivial degree as to the kind or proportion of their ingredients, are adapted to widely different uses. Thus the large grained salt is peculiarly fitted for the packing of fish and other provision, a purpose to which the small grained salts are much less suitable. Their different powers then of preserving food must depend on some mechanical property; and the only obvious one is the magnitude of the crystals, and their degree of compactness and hardness. Quickness of solution, it is well known, is pretty nearly proportional, all other circumstances being equal, to the quantity of surface exposed. And since the surfaces of cubes are as the squares of their sides, it should follow, that a salt, the crystals of which are of a given magnitude, will dissolve four times more slowly than one, the cubes of which have only half the size.

What is the cause of the difference of quality in salt?

Not the chemical composition,

but the size of the crystals.

That kind of salt then which possesses most eminently the combined properties of hardness, compactness, and perfection of crystals, will be best adapted to the purpose of packing

Practical application.

packing fish and other provision; because it will remain permanently between the different layers, or will be very gradually dissolved by the fluids that exude from the provision; thus furnishing a slow, but constant supply of saturated brine. On the other hand, for the purpose of preparing the pickle, or of *striking* the meat, which is done by immersion in a saturated solution of salt, the smaller grained varieties answer equally well; or, on account of their greater solubility, even better.

Specific gravity.

With the hardness or strong aggregation of the several varieties of salt, it seemed to me not improbable, that their specific gravity might, in some degree, be connected. The exact determination of this property in saline substances is, however, a problem of considerable difficulty, as will sufficiently appear from the various results which have been given, with respect to the same salts, by different experimentalists. Thus Muschenbroek makes the specific gravity of artificial muriate of soda to vary from 1918 to 2148, the mean of which is 2033. Sir Isaac Newton states it at 2143, and Hassenfratz at 2200*. All that was necessary for my purpose was an approximation to the truth; and the introduction of a small errour could be of no importance, provided it were the same in every case, since the comparison would still hold good.

Rock salt.

The specific gravity of rock salt there can be little difficulty in determining with precision. A piece of this salt†, of such perfect transparency, that I had reserved it as a cabinet specimen, weighed in the air 513 grains, and lost, when weighed in alcohol, 194 grains. The alcohol, at the temperature of 56° Fahrenheit, had the specific gravity of 820, and hence that of the salt may be estimated at 2170. Another specimen considerably less pure, and more approaching to a fibrous fracture, had the specific gravity of 2125 only.

Mode of ascertaining the specific gravity of different kinds of salt.

For ascertaining the specific gravities of artificial varieties of salts, I used a very simple contrivance. It consisted of a glass globe about 3½ inches diameter, having a neck or

* Annales de Chimie, vol. XXVIII, p 11

† Foliated rock salt of Jameson. See his Mineralogy, vol. II, p. 10.

neck,

neck 10 inches long. Sixteen cubic inches of water (each 252½ grains at 60° Fahrenheit) filled the whole of the globe, and about half an inch of the lower part of the neck; and from the line where the water stood in the instrument, it was accurately graduated upwards into hundredth parts of a cubical inch. Into this vessel I poured exactly sixteen cubic inches of a perfectly saturated solution of common salt; and then added 400 grains of the salt under examination, washing down the particles that adhered to the neck by a portion of the liquid, which had been previously taken out of the globe for the purpose. As much as possible of the air which adhered to the salt was dislodged by agitation; and the increase of bulk was then observed.

Care was taken, that the salts were all of equal temperature and dryness, and that no change of temperature happened during the experiment.

	Hundredths of a cub. in.	Hence its specific grav was*	Specific gravity of salt.
400 grains of the less pure kind of rock salt, broken down into small fragments, filled the space of	75	2112	
400 grains of stoved salt	75	2112	
400 do. (another sample)	76	2084	
400 do. common salt	76	2084	
400 large grained fishery salt	83	1909	
400 do. (another sample)	83	1909	
400 St. Ube's	82	1932	

If the above mode of determination at all approach to correctness, it would appear, that the specific gravity of rock salt is diminished, by being broken into small fragments, from 2125 to 2112, probably in consequence of the quantity of air which the fragments envelope, and which cannot be entirely separated by agitation. From the numbers given in the last column, it is evident that the smaller grained salts are specifically heavier than those which are composed of larger and more perfect crystals. A difference of only one or two hundredth parts of a cubic inch is perhaps entitled, in a process of this kind, to little reliance; and I do not therefore regard it as indicating any material difference in the specific gravity of the first four, or last

* Distilled water at 1000 being taken as the standard.

three

three salts submitted to experiment. But when the difference amounts to eight hundredths, as between the small and large grained salt, it may safely be imputed to an inferior specific gravity in that species, which occupies so much greater a proportional bulk*.

British salt at least equal to foreign.

The last series of experiments proves decisively, that in an important quality (viz. that of specific gravity), which is probably connected with the mechanical property of hardness and compactness of crystals, little or no difference is discoverable between the large grained salt of British, and that of foreign manufacture. If no superiority, then, be claimed for British salt as applicable to economical purposes, on account of the greater degree of chemical purity which unquestionably belongs to it, it may safely, I believe, be asserted, that the larger grained varieties are, as to their mechanical properties, fully equal to the foreign bay salt. And the period, it may be hoped, is not far distant, when a prejudice (for such, from the result of this investigation, it appears to be) will be done away, which has long proved injurious to the interests and prosperity of an important branch of British manufacture.

(To be concluded in our next.)

IX.

On the Proportions of the Elements of some Combinations, particularly of the Alkaline Carbonates and Subcarbonates: *by Mr.* J. E. BERARD†.

Component parts of salts should be ac-

"THE accurate determination of the component parts of saline substances is of the more importance, because it is

* Mr. Hassenfratz seems to have suspected, that a difference in the specific gravity of the same salt may be occasioned by a variation in its mode of crystallization. *De la Pesanteur spécifique des Sels;* Ann. de Chim. XXVIII, p. 17.

† Abstracted from Ann. de Chimie, vol. LXXI, p. 41.

is employed as the basis of other chemical analyses. Berthollet, who has sought to determine some of these in his late papers, was desirous, that they should be carried to the highest degree of accuracy; and invited me, to resume the subject, reiterating the experiments, varying the methods, and taking the greatest care to avoid every source of errour. This I have endeavoured to do, and at the same time I extended my observations to a greater number of compounds. curately determined.

"As these determinations depend particularly on the accuracy of the weights, I think it necessary to say, that I always used a balance made by Fortin, which, when loaded with a kilogramme [2¼lbs. nearly], is sensible to a milligramme [0·01544 of a grain, or one millionth of the load]."

After this introduction, Mr. Berard, in a pretty long paper, gives the detail of his experiments; but as these would occupy much room to little purpose, I shall pass them over, merely giving the tabulated results, with which he concludes.

Table of salts.

Salts.	*Base.*	*Acid.*	*Total.*
Muriate of potash - - - - - -	66·66	33·34	100*
——— of soda - - - - - - - -	57·00	43·00	100
Sulphate of barytes - - - - -	67·70	32·30	100†
——— of potash - - - - -	57·24	42·76	100
——— of soda - - - - - - -	47·22	52·78	100
Nitrate of potash - - - - - -	48·64	51·36	100
Carbonate of potash - - - - -	53·81	46·19	100‡
——— of soda - - - - - -	44·38	55·62	100
Subcarbonate of potash - - -	70·21	29·97	100
——— of soda - - - -	62·53	37·47	100

* Observations on the Proportions of the Elements of Compounds, Mém. d'Arcueil, vol. II.

† Ibid.

‡ I suppose the carbonate of potash, as well as the other salts, to be totally deprived of water."

X.

On Chemical Printing, and particularly on the Progress of this Art in Germany: by Mr. MARCEL DE SERRES, *Inspector of Arts, Sciences, and Manufactures**.

Vienna, october the 17th, 1809.

History of the art.

THE art of printing from stone, known in Germany by the name of *chemische druckerey*, "chemical printing," originated in Germany; whence it spread first into England, then into Italy, and lately into France. It was invented by Aloys Senefelter, who was born at Prague, in Bohemia. Nine years ago he obtained of the king, formerly elector of Bavaria, an exclusive patent for its use for thirteen years; but he afterward sold the right to his brothers. Some time after Senefelter sold his right also to Mr. Andrew von Offenbach, who at present exercises the art in England. In 1802 he came to Vienna, to solicit a patent, and in 1803 he obtained one from the emperor of Austria for ten years. Changing his mind, he parted with this patent to Messrs. Steiner and Krasnitzki, returned to Bavaria, and set up a chemical printing office at Munich in partnership with some other persons. Messrs. Steiner and Krasnitzki still continue the business at Vienna, under the patronage of the counsellor of regency Startl von Luchsenstein, who is a zealous promoter of every useful undertaking.

At the chemical printing office at Munich the art has attained the greatest perfection, that of Stutgard apparently being of much less importance. Mr. Chauvron was the first who obtained a patent in France for printing or engraving on stone, and Mr. Guyot-Desmarets did not attempt it till after him.

The processes employed are simple, but as only a brief account of them has yet been given, it may be of use at least to make known those followed in Germany.

Three different methods.

In the chemical printing office at Vienna three different

* Abridged from the Annales de Chim., vol. LXXII, p. 202.

methods

methods, are employed; but that termed in relief is most frequently used. This is the general mode of printing music.

The second method is the sunk. This is preferred for prints.

The third method is the flat, or neither raised nor sunk. This is useful for imitating drawings, particularly where the impression is intended to resemble crayons.

For printing or engraving in this method a block of marble is employed, or any other calcareous stone, that is easily corroded, and will take a good polish. It should be two inches or two inches and half thick, and of a size proportioned to the purpose for which it is intended. A close texture is considered as advantageous. The stone used.

When the stone is well polished and dry, the first step is to trace the drawing, notes, or letters, to be printed, with a pencil. The design is not very conspicuous, but it is rendered so by passing over the strokes of the pencil a particular ink, of which a great secret is made. This ink is made of a solution of lac in potash, which is coloured with the soot from burning wax. This appears to be the most suitable black for the purpose. When the design has been gone over with this ink, it is left to dry, which commonly takes about two hours; but this depends much on the temperature and dryness of the air. Process described.

After the ink is dry, nitric acid, more or less diluted according to the degree of relief desired, is poured on the stone; and corrodes every part of it, except where defended by the resinous ink.

The block being washed with water, an ink similar to that commonly used for printing is distributed over it by means of printers balls, a sheet of paper disposed on a frame is laid on it, and this is pressed down by means of a copper roller, or copper press. The beauty of the impression will necessarily depend on that of the design. These copper presses are very ingeniously constructed in Germany, and easily worked. Their weight is proportional to the method of printing used. Mode of printing.

When the desired number of impressions is taken off, and the work is not intended to be usedany more, the stone is polished The stone may be use for other works.

polished anew; and thus it may be made to serve for thirty or forty different works.

Chalk or sunk method.

The sunk, or chalk method differs from that termed in relief only in having the stone much more corroded by the nitric acid. This is chiefly employed for prints, and has the advantage of remedying that uniformity of tint, which is common to prints from the chemical press. It is natural, that the higher parts should take less of the ink, and the lower parts more, so that the impression has less monotony; a defect hitherto seemingly inherent in this mode of printing. For this method too the rollers must be stronger and heavier.

Its use.

As this method is more expensive, it is given up: yet for prints, where some degree of effect is required, and more clearness, it is to be preferred. In this method nearly pure nitric acid is employed. Indeed when the art was first invented, pure nitric acid was always used; but soon after, to save expense, it was diluted with water; and since that it has been employed more or less diluted, according to the effect wished to be produced on the stone.

Relief.

For the method in relief, as it is called, nitric acid with half water is used.

Flat method.

In the flat method less nitric acid is used. It is not to be supposed, that the surface is quite plain in this way; but the lines are very little raised, so that they can scarcely be perceived to stand above the ground but by the finger.

Application.

The works executed in stone are; 1, imitations of wood cuts: 2, imitations of engravings in the dotted manner: 3, drawings: 4, music: 5, all kinds of writing: 6, maps: 7, copperplates.

Advantages.

The advantages of this method are, that it has a peculiar character, which cannot be represented by any other mode, while it gives a tolerable imitation of other methods; and

Celerity.

still more the celerity, with which it can be executed. A subject, that an artist could not finish in five or six days on copper, may be engraved on stone in one or two. While a copperplate printer is taking off six or seven hundred impressions, two thousand may be printed in this way. A copperplate will scarcely give a thousand good impressions, while in this way several thousands may be taken off, and the last be as perfect as the first. Thirty thousand have been taken

Number of impressions.

off

off one design at Vienna, and the last was as beautiful as the first. This is intended to be carried still farther, fo the purpose of printing bank notes. The most expert music engraver can scarcely execute four pages of music on pewter in a day, but the engraver on stone can finish twice as much in that time. Bank notes.

To enter into the particulars would take up too much room, but experience has shown, that this mode saves two thirds of the expense of engraving on copper or pewter. Cheapness.

After having mentioned the advantages of printing from stone, it is just to point out its disadvantages. These are, the difficulty of giving that diversity of tone, which is admired in engravings. Thus for instance, the finest prints that this art has yet produced are unquestionably those, that have been executed at Munich* from those celebrated drawings, which from a whim, in which painters are apt enough to indulge, Albert Durer made in a prayerbook. These prints are executed with spirit, and the stroke is frequently clean; but it is uniform, so that the print is somewhat gray and monotonous. The difference is still better perceived, on comparing these prints with those etched by the different masters themselves. Disadvantages. Best specimen.

The same inconvenience is found in music, the uniformity that prevails rendering the music less easy to read.

We must not too hastily conclude however, that this new art is not important: we should endeavour to find means of remedying the inconveniences, that appear to arise from the mode employed. If such means be discovered, which we may hope from experience showing, that the manner of applying the acid and of drawing upon the stone are the points most important to improve, this mode of printing will combine a saving both of time and expense. The great number of copies too, that may be taken off, is not one of its least advantages. Improvement to be attempted.

It remains now to notice the differences, that appear to exist in the chemical printing offices of different cities. At Milan a little nitric acid is poured over the stone, as at Differences in different places.

* Albrecht Durers Christlich mythologische Handzeichnungen. Strixner, Munich, 1808. Different inks have been used for prints, as black, red, violet, and green.

Vienna: but it is said, that they cannot take off above five hundred impressions. This must be owing to the nature of the stone employed, which is procured from Verona.

nch mode. Chauvron, the first who set up a chemical printing office at Paris, after having traced the design on stone with a resinous ink, merely wets it with water, and wipes off the water from the design. Printing ink is then applied by beating in the common way with balls; and, as this does not adhere to the wet stone, the resinous strokes only produce an impression. Chauvron is said to have printed a great deal of music in this way.

Use of nitric acid. We must observe, that, where nitric acid is not used, the prints will never be so fine, and so many impressions cannot be taken off. The use of nitric acid therefore cannot be too strongly recommended.

XI.

Observations on Dr. BOSTOCK's *Remarks upon Meteorology, by* LUKE HOWARD, *Esq.*

To WILLIAM NICHOLSON.

RESPECTED FRIEND,

Natural history and nomenclature of the clouds. IT is about eight years since I brought forward an essay containing the Natural History of the Clouds, with a nomenclature formed chiefly from their visible structure. This work, imperfect as it was, met with a favourable reception. In the Annual Review for 1804, Dr. Bostock bestowed upon it a pretty long critique, on the whole favourable to the adoption of the nomenclature, but which he seems to have since forgotten. He now says, "I am not unaware, that a scientific nomenclature for the appearance of the clouds has been attempted by Mr. Howard, but I hope I shall not be accused of presumption, if I give it as my opinion, that his set of names is much too confined to be of any great use, and that the hypothesis on which he proceeds is not entirely correct."

Nomenclature objected to.

There

There was surely no occasion to deprecate censure. In common with the whole public, Dr. B. has just the same liberty to reject the nomenclature, which I had to propose it; and from the care with which he avoids using a single term of mine, I perceive he intends to avail himself of this. Should it prove, that it is to make way ultimately for something of his own, I hope to entertain his attempt with the impartiality which the interests of science require. In the mean time I may be allowed to say something in behalf of my system, which I shall do the more readily, as it will give an opportunity of meeting his former objections. This free for any one.

The system in question imposes names on *seven* modifications of cloud, only: but these divisions are, in effect, so many genera, some of which comprehend many species; if indeed it be right to apply these terms to the evanescent forms of our subject, which occur at uncertain and sometimes very distant intervals, and run through gradations so delicate, and combinations so varied, that the leisure of a long life might not more than suffice accurately to observe and describe them. In the interval since 1804, I have not seen sufficient cause to disturb my original plan by adding or suppressing modifications. It is not that I am vain enough to deem it perfect, but I believe it answers the intended purpose. The system explained.

In the present infant state of meteorology, and until these or some other general distinctions shall be adopted and practically understood, I do not apprehend it will be of advantage to multiply terms, especially such as, being referable to no system, are applied to complicated appearances, which must be analysed before they can be clearly understood. Multiplication of terms not desirable,

The materials, however, for specific distinctions, will accumulate in proportion as accurate observers increase, and it is matter of indifference to me, who first imposes appropriate names on well ascertained and well defined species or combinations. till more specific distinctions are noted.

As to the erroneous hypothesis, on which I am said to proceed, it must be that of Dalton on the constitution of the atmosphere, which I used in aid of my explanations, or rather *speculations*, on the causes of the phenomena. If Dr. B. will turn to the article *Cloud*, in Rees's Cyclopædia, Mr. Dalton's hypothesis

he

not essential to the natural history.

he will there find the natural history detached from this so much combated hypothesis, and resting solely on its proper basis, that of long continued and attentive observation of the phenomena, I undertook to describe and classify.

The terms of Dr Bostock only objectionable.

In objecting, as I shall next be obliged to do, to the whole method of Dr. Bostock in forming his terms, I would not be understood to detract from the value of his original observations. A continuation of them in more clear and definite phraseology cannot fail to prove useful to the science. To use new terms is in effect to propose them for use, or to introduce a new nomenclature. Those of Dr. Bostock are liable to objections more apparent than that of being "uncouth;" they are inaccurate and imperfect. To make this appear we must examine them singly, which will also give opportunity for the discussion of several facts, to which they relate: and first, the *arc*, past, present, and future.

The arc.

This particular formation of the clouds must have attracted the notice of most observers of the sky. The structure is not very clear from the Doctor's account. He says, the lines converge to a point in the horizon, and such is certainly the appearance to the eye. In the article *Cloud* before mentioned, the reader will find it thus noticed: "It (the cirrus) is sometimes spread horizontally through a vast extent of atmosphere; the whole breadth of the sky being insufficient to show where it terminates. In this case, its *parallel* bars appear, by an optical deception, to converge in opposite points of the horizon." These parallel bars are the *linear* arc of Dr. B. When the cirrus is compound, we have his *feather'd* arc; and in varieties of the cirrocumulus, the *mottled* and *wreathed* arc. Now, although each of these linear clouds may, in some sense, be an arc, as following the curve of the Earth's surface, yet it is not this curve which the eye perceives, but a system of converging lines. The term *arc* therefore, applied to such a system, seemed so unphilosophical, that I could not imagine whence it came; till I remembered that the name of Noah's *ark* is given, in popular language, to an assemblage of clouds, the nature of which is not very obvious from the name. Recollecting this I was led to suppose, that Dr. B. had borrowed the sound, though not the sense, of this popular allusion to the

orderly

orderly *converging march* of the animals assembling to Noah. Be this as it may, the observations on a connection between this phenomenon and the variations of the wind are highly interesting. In verifying them by future cases it will be proper to ascertain a fact, which seems to have been taken for granted: viz. that the lines of cloud continue to range between the same points, while the wind below is veering: for it is possible, extensive as they are, that they may undergo a change of direction. The phenomenon is rare, in this champaign district, and I believe frequent near the sea, or among mountains. I must protest, convenience notwithstanding, against calling a thing *past* or *future*, which is *present*, and the subject of actual observation.

Revolving, & reverse revolving day.

A *revolving day*. A *reverse revolving day*. These are not more happily formed. A reader meeting with the latter, and not having the vocabulary at hand, might attempt to solve the puzzle by considering, that day and night are produced by a revolution of the Earth on its axis. A *reverse* revolving day must then be one in which, that revolution being reversed, the sun had risen in the west and set in the east! To the terms a direct or a reverse revolution of *the wind* there can be little objected. These are prognostics of long standing, and like others, taken singly, are not infallible. We had a *reverse revolution* here on the 4th inst. (as I recollect) followed by a dry, though cloudy day, with an easterly current.

Sour day.

The term *sour day*, I think, we may leave to the painters. It is matter of taste rather than of science, to define it. *Dissolving air* or *constitution* is obviously connected with an hypothesis, and that an exploded one. Solution, in its only proper sense (as the result of a chemical attraction between the ponderable base of air and water) has been found to enter for little or nothing into the case of evaporation.

Dissolving air.

Various epithets of clouds.

Round clouds, shaded clouds, piled clouds, rolling clouds, white clouds on a gray ground, &c. I do not see the advantage to science of these attempts to substitute description for definition. The *piled* cloud will be the *shaded*, or some other, when it comes toward the zenith; and the *shaded* will be light in the horizon opposed to the sun. As for the *rolling* cloud I have not yet detected it; and it seems too poeti-

cal,

cal, if, as I conjecture, it is so named, because its parts, if solid, would roll when on an inclined plane. There is a rapid introversion or *curling* of the tops of the cumulus while the cloud is rapidly evaporating, or rather perhaps *dispersing*, which is a pretty good indication of approaching rain; but the *mass* in this case continues well balanced.

Cumulus.

I shall here repeat (in opposition to the assertion of Dr. Bostock, Annual Review, 1804, p. 900,) that the cumulus *is a production of the day*; and that it is not "frequently," but on the contrary very rarely "seen in the night." He may have taken for it the remnants of an evening cumulo-stratus, or the parts of a dense and low cirrocumulus, as they often appear by moonlight; but if he will take up again the definition, and repeat his observations, I think he will not meet with it after sunset, save by chance in a thunder group.

Tufts, lines, flocks.

Tufts, lines, flocks. These are varieties of the cirrus; but cirrus is a *Latin* term, and to such, it seems, the Doctor has at present a strong objection, founded on this reason, that the farther improvement of the science will probably, in a considerable degree, depend upon the observations of the *unlearned* (An. Rev. 1804. p. 898). But surely the unlearned can learn, as they have done heretofore. *Alphabet*, which is greek curtailed, is as well understood as *a, b, c*; *zenith* and *nadir* are Arabic; and as for *Latin*, our Scotch gardeners can talk it fluently (I will not say classically), and we have senior, junior, maximum, minimum, prospectus, index, finis, quota, quantum, vacuum, and a hundred more, which the unlearned use daily, without suspecting how learned they are. There is a great advantage, ultimately, to the learner, however small his capacity, in giving him terms, the use of which must be acquired with some labour by means of definitions. It packs his knowledge, if I may be allowed the phrase, in much less room, and enables him to find it with ease when wanted. But I had another motive, of still greater importance, to the commencement of a latin nomenclature. I conceived, that the subject would interest observers on the continent; and that, by means of a language common to all, the observations of each country might with ease be communicated to the rest; which they never could be,

Advantage of terms, the meaning of which must be learned.

A latin nomenclature intelligible on the continent.

be, with accuracy, by such a nomenclature as I have been examining. I hope, therefore, that my fellow-labourer, whose production I have, in my turn, been criticising, will accede to this principle, that, in forming a nomenclature for meteorology, the terms which it shall be needful to establish and define (in addition to common words on the subject) shall be carefully formed from the Latin, in which there exist already a number of words that are perfectly apposite. Those who are precluded from acquiring these, may be left, as in other sciences, to the use of their peculiar synonyms, which the student will find it no very difficult task to attach to their correspondent terms in his own series; for, if I have a just conception of the extent of the vulgar nomenclature, it would form but a limited collection.

Transparent atmosphere.

I [illegible] not now enter into every consideration, which has occurred on the perusal of Dr. Bostock's elaborate communication; but before I conclude I must notice another phenomenon, in the explanation of which I differ from him. He speaks of a *transparent* condition of the atmosphere, as one of the most infallible signs of a change of weather, and as seldom lasting more than a few hours. I am convinced from several points in his description, and particularly from the *varnish* which it is said to throw upon any large expanse of water, that he intends a condition of the atmosphere, in which, though transparent below, it is more opaque than common in the higher part. There exists in those regions at such times a quantity of water in a peculiar state of diffusion, giving a strong milky opacity to the twilight, the reflection of which occasions the varnished appearance to which he adverts. The whole quarter above the sun, after it has set, has sometimes even a lively *pink* tinge, and in this case a thunder storm ensues. The *lilac* or *violet* band, spread round the horizon, is merely the colour of the falling dew; and this belongs rather to that perfectly transparent state of the atmosphere, which accompanies our easterly winds in the spring, and is so far from being transient, that there is no state in which it is found to continue for a longer time the same.

I am, respectfully, thy friend,

LUKE HOWARD.

Plaistow, Essex,
16th of 6th Month, 1810.

XII.

XII.

Experiments on the comparative Powers of Cylinder and Plate electrical Machines, and on a means of doubling, trebling, or quadrupling their charging Power: by Mr. JOHN CUTHBERTSON *and Mr.* G. J. SINGER. *Communicated by Mr.* SINGER.

Best shape for electrical machines doubted.

THE opinion of electricians has been at various times much divided with regard to the best form of an electrical machine. Globes, spheroids, cylinders, and plates, have been alternately employed and recommended by various experimenters: but the last two forms have been recently by far the most prevalent. The earlier electricians produced but slight electrical effects; and though some attempts were made at improvement, by alterations in the structure of the apparatus, and by the adoption of machinery; the tests of electrical action at that time known were far from adequate to any accurate comparison of their relative merits. The much improved construction of the cylinder machine by Mr. Nairne, the experiments on electrical excitation by Mr. Nicholson, and the structure of the unrivalled Harlem apparatus by Mr. Cuthbertson, are the circumstances that have most enlightened this subject; and it is from these sources, the various opinions, that have been entertained in this respect, have principally originated.

Formerly but slight effects produced.

Improvements.

Effects influenced by slight circumstances.

The influence of the hygrometrical state of the atmosphere on electrical experiments, and the considerable diversity of effect produced by slight alterations in the disposition of the apparatus, will convince every electrician, that no just comparison of the merits of any two instruments can be made, unless they are employed in one situation, at the same time, and under similar circumstances. These essential requisites to just observation have been attended to as strictly as possible in the following experiments, made with the express intention of obtaining every useful information on this particular subject.

These attended to in the following experiments.

Instruments employed.

The instruments employed for comparison were a cylinder of 14 inches diameter (with a multiplying wheel and pulley,

pulley, the proportionate diameter of which are as four to one), and a single plate machine of 24 inches diameter, turned by a single winch, as usual. The management and excitation of each machine were undertaken by the individual who had constructed it, and an equal advantage was thus afforded to both. To ensure greater accuracy, the results noted were in all cases the mean result of repeated experiments; without this precaution, the sources of anomaly would be much more frequent and numerous.

Best test of electrical action.

Mr. Cuthbertson's experiments (published in this journal, 4to edition, vol. II, page 525: and in his Essay on practical Electricity and Galvanism, parts 6 and 7) have decisively proved, that no known test of electrical action is so uniformly accurate, as that of the gradual increase of charge, conveyed to a known measure of coated surface, measured by his electrometer, and by the fusion of determinate quantities of wire. These were consequently the means most frequently employed, and on which the greatest reliance was placed; but in the progress of inquiry recourse was also had to every known means of measuring comparative quantities.

But every known means here employed.

Difficulties at first experienced surmounted.

In our first experiments much difficulty was experienced, in consequence of the variation produced in charges of high intensity, by the different arrangement of the conductors, (a circumstance which cannot be avoided in the present form of the instruments): but by altering the size and situation of the terminating balls, and modifying the charge, the production of tolerably uniform results was at length accomplished. The mean result of nearly one hundred distinct trials tended to prove, that the charging power of the two instruments was precisely equal; and this conclusion was rendered indisputably accurate by the following experiments.

The two machines of equal power,

proved by experiments.

Exp. 1. A battery of 15 jars, exposing about 17 square feet of coated surface, was connected with Mr. Cuthbertson's electrometer, the slider on the arm placed at 15 grains. 4 feet of Iron wire, $\frac{1}{100}$ of an inch diameter, were placed in the circuit. The battery was first charged by the cylinder. 120 turns of the wheel effected the discharge, the wire was rendered red hot and fused into balls.

Exp. 2.

Exp. 2. The same arrangement as in the former experiment. The charge communicated by the Plate. The discharge was produced by 138 turns, and the wire fused as before.

Exp. 3. The apparatus arranged as in the former experiments. The *two* machines employed together to communicate the charge. The electrometer discharged itself in 65 turns; the wire was melted as before.

The effects equal,

In this last experiment the effect was produced in exactly half the number of turns by the *two* instruments, that had been required by the most favourable action of *one*; and it is perhaps impossible to conceive a more complete proof of the similarity of their powers. The 48 inches of wire fused in these experiments was afterward found to be a tolerably exact measure of the force of the charge employed; for on repeating the experiment with 49 inches of the same wire, it was barely rendered red hot.

and the wire fused a good measure of the charge.

The plate machine more easily worked.

Equal effects being produced by either machine in equal times, it became necessary to ascertain, whether any difference existed in the power required to put them in motion. The handles were placed in a horizontal position, and weights applied to them. 8lbs. troy were sufficient to move the handle of the plate machine, but it required 14lbs. to produce the same effect with the cylinder.

Some additions necessary.

Here our first series of experiments terminated; the results we had obtained had shown the necessity of some addition to our apparatus, and we mutually agreed to defer the farther prosecution of the subject, till these should be supplied. In the mean time, some circumstances, which had occurred in these experiments, continued to occupy our consideration separately. Mr. Cuthbertson, as well as myself, had been much surprised at the uniform action of the two instruments; and the active discrimination with which he considered the subject soon offered a means of increasing and extending their powers, to an almost indefinite extent.

Construction of the cylinder.

The cylinder machine we employed is constructed on a plan I believe to be peculiar to myself; it is entirely insulated, in the manner of Mr. Nairne's; but the motion is communicated by multiplying wheels with a silk cord,

cord, instead of an insulating winch. I adopted this plan to prevent the effect of that undulation, which has been described by Mr. Nicholson as peculiar to cylinders (in consequence of their irregular surface, which occasions an unequal pressure of the rubber): and to obviate this inequality still more effectually, the rubber is so formed, that the back of it acts as a horizontal spring; which keeps up a steady, and nearly uniform pressure, without the imperfection of shortening the negative insulation. I have never found this machine vary materially in its action, although I frequently employ it through a whole course of lectures without any fresh application of the amalgam; nor do I find that its power of excitation is at all affected by a moist atmosphere.

Mr. Cuthbertson conjectured, that my machine owed much of its power to the multiplying wheels; and having noticed, that the cylinder made four revolutions for one of the wheel, requested me to try its action with a simple winch. The pressure remaining the same as in the former experiments, it required (as might have been expected) four times the number of turns to produce any given effect. The friction however was materially less, 2¼lbs. troy being sufficient to move the handle from its horizontal position.

Its power increased by the multiplying wheels.

These greatly increased the friction.

Soon after these experiments Mr. Cuthbertson informed me, that he had discovered an improvement in the plate machine; by which its charging power might be doubled, trebled, or even quadrupled, without any inconvenience but that of increased friction. I could not at first conceive by what means this might be effected; but after some consideration concluded, it could only be accomplished by the application of multiplying wheels to the plate machine; and Mr. Cuthbertson then told me, this was the idea that had occurred to him.

Applied to the plate machine.

It was agreed, that experiments in proof of Mr. C.'s invention should be made at the conclusion of our comparative experiments; and the requisite apparatus being completed, we resumed our inquiry. Before this second series of comparative experiments, I made some alteration in the rubber of the cylinder, and furnished it with new silk; suspecting, that three years constant wear might have so far deranged

Comparative experiments.

Rubber altered, & new silk applied,

deranged it, as to render a comparison with the new plate machine unfair. The result answered my most sanguine expectation. The rubber and its new silk being completed, I found the power of the cylinder machine increased one third; for a trial jar, which had before exploded four times in one turn of the wheel, now afforded six explosions in one turn. The experiment was several times repeated, and the result was uniformly the same.

Increased the power of the cylinder.

The following comparative experiments were then made:

Plate machine.

Exp. 1. Length of spark, from the prime conductor of the plate machine, to a two inch ball connected with the ground. The greatest interval, obtained by gradually increasing the distance between the balls, till the limit at which the spark would pass was ascertained, six inches and a half.

Cylinder.

Exp. 2. The same arrangement with the cylinder. The greatest striking distance eight inches and a half. With larger balls, a longer spark could be obtained; I have occasionally procured them upwards of 12 inches; but it is requisite for this purpose to turn the machine very slowly, which occasions undulation.

Lane's electrometer not to be depended on.

Experiments were next made on the charge of a jar fitted up with Lane's electrometer. The following table exhibits the most uniform results obtained in a great number of experiments; and may show how little reliance is to be placed on the accuracy of this test.

Jar of 168 square inches.

Plate Machine.		*Cylinder.*	
Length of spark.	Number of turns required for the discharge.	Length of spark.	Turns of the wheel required for the discharge.
¾ of an inch	2	⅝ of an inch	1½
1 inch	4	1 inch	2¼
2 inches	8	2 inches	4
5¼ inches	8	5¼ inches	6

Cuthbertson's electrometer.

The anomalies in the experiments with Lane's electrometer are not more remarkable than the uniform accuracy of the following, which were made with Mr. Cuthbertson's most excellent electrometer.

Jar

Jar of 168 square inches.

Plate Machine.		*Cyl*	
Slider on the arm of electrometer at	Turns of the winch required for the discharge.	Slider on the arm of electrometer at	Turns of the wheel required for the discharge.
15 grains	4	15 grains	3
20 grains	5	20 grains	$3\frac{1}{2}$
25 grains	6	25 grains	4
30 grains	7	30 grains	$4\frac{1}{2}$
35 grains	8	35 grains	5

No higher charge was employed; for this could not have been obtained without breathing into the jar, which might have occasioned irregular results.

A battery of 15 jars exposing 17 square feet of coated surface was next employed, and the following results were obtained. Further experiments.

Plate Machine.		*Cylinder.*	
Slider on the arm of the electrometer at	Turns of the winch required for the discharge.	Slider on the arm of the electrometer at	Turns of the wheel required for the discharge.
10 grains	75	10 grains	63
15 grains	102	15 grains	70

The battery was next charged by the cylinder, turned with a simple winch; the slider on the arm of the electrometer remained at 15 grains, the discharge took place in 246 turns. Hence it appears, the advantage gained by the cylinder with its multiplying wheels is not in the exact proportion of the increased number of turns, the intensity being somewhat diminished by the rapidity of the motion: for $70 \times 4 = 286$, and only 246 were required, a deficiency of nearly $\frac{1}{8}$th. The increased power not exactly proportioned to the velocity of the motion.

Multiplying wheels were now applied to the plate machine, the ratio of increase was as follows: Plate machine.

Battery of 15 jars.

Slider on the arm of electrometer at 10 grains

Turned by a simple winch effected the charge in	Turned by multiplying wheels 2 revolutions to 1.	By ditto, 3 revolutions to 1.	By ditto, 4 revolutions to 1.
75 turns.	42 turns.	28 turns.	19 turns.

These

Utility of this improvement.

These experiments sufficiently prove the utility and importance of Mr. Cuthbertson's improvement of the plate machine, by the application of multiplying wheels; for when allowance is made for some defects in the machinery we employed, (which had only been mounted in a rough manner for trial), it will be found, that the charging power increased so nearly in proportion to the number of revolutions; as to exhibit in no instance a deficiency of more than $\frac{1}{8}$th: and in the last experiment with the plate machine, the diminution is only $\frac{1}{10}$th; although the original charging power is quadrupled. Thus in the three experiments in which the motion of the plate was accelerated; $42 \times 2 = 84$; $28 \times 3 = 84$; and $19 \times 4 = 76$. The numbers required by the simple winch was 75, and 75 is to 84 nearly as 7 to 8.

Principle capable of being carried farther,

As there is no reason to suppose, that our experiments have reached the limit, to which this charging power may be increased; it is fair to conclude, that, by the proper application of a moving power, the quantity of electricity given out by any machine in a determinate time may be *doubled, trebled, quadrupled*, or even increased *six* or *tenfold*. The discovery of this principle is therefore of the highest importance, as it offers the most effectual and ready means of obtaining a very considerable accumulation of the electric fluid; a circumstance of considerable interest in the present state of electrical and chemical inquiry.

and of great importance.

Comparative advantages of the two machines.

With respect to the comparative advantages of the different instruments, much might be said; at present it may suffice to state only those facts, which are of general utility. When we first endeavoured to ascertain the power required to put each machine in motion, it appeared, that the cylinder required 14lbs. and the plate only 8lbs. It was afterward found, that this disparity arose in part from an inequality in the levers, by which they were moved; when these were made equal, the difference was much less considerable, 10lbs. were required for the cylinder, and 8lbs. for the plate; and this difference in their friction will not be found more than commensurate to the difference in their acting power.

The

The principal advantages in the cylinder are, 1st, the positive and negative powers are obtained in equal perfection; 2d, it has but one rubber to keep in order; 3dly, it is less liable (from the security of its form) to accidental fracture, than the plate; 4thly, its insulation is more perfect; and 5thly, from the peculiarity of its structure, larger multiplying wheels may be employed, and thus a considerable diminution of friction be obtained. Those of the cylinder.

The advantages of the plate machines are, 1st, they are less expensive than cylinders of equal power; 2dly, they occupy less room; 3dly, may be constructed of a much larger size, as instanced by Mr. Cuthbertson's large machine at Harlem; 4thly, several plates, to act jointly, may be more easily combined, than several cylinders could; 5thly, the multiplying power may be applied to them to a much greater extent, than it could to cylinders, without rendering the motion too rapid; 6thly, plates of equal diameters may be made to act with a uniform and equal degree of power, a circumstance seldom attained by cylinders. Those of the plate machine.

These are the only conclusions the present state of our inquiry seems to warrant; many experiments remain to be made, and when a sufficient number of these are completed, I shall not delay the communication of them to the public. Farther experiments to be made.

3, *Princes street, Cavendish square,*
June 13th, 1810.

XIII.

Researches on Acetic Acid, and some Acetates: by RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A. &c.**

THE identity of the acids contained in vinegar and in the product of the distillation of verdigrise, is now generally admitted; and the terms acetous acid and acetites have No acetous acid.

* Annales de Chimie, vol. LXIX, p. 5.

been finally erased from the list of chemical substances, in which, for more reasons than one, they ought never to have been inserted.

Distillation of metallic acetates.

Though several chemists have already turned their attention to some of the subjects of the present inquiry, I believe I have some new facts to add on the distillation of metallic acetates; and without any intention to diminish the claims, which Messrs. Courtenvaux, Lauraguais, Monnet, Lassonne, Edenzel, Berthollet, Chaptal, Proust, Higgins, Pelletier, Adet, Darracq, Dabit, Trommsdorff, Derosne, &c. have to our acknowledgments, I shall proceed to relate the whole of my researches, begun in March 1803, but which I was unable to finish before the present moment (January the 11th, 1808.)

English vinegar distilled.

In a glass retort I carefully distilled two quarts of vinegar, made in England from malt. Its specific gravity had become 1·0042. I saturated it with carbonate of potash, and distilled it again to dryness. What remained in the retort was acetate of potash coloured by vegetable matter. The liquid that came over was perfectly clear and colourless, and retained a slightly spirituous smell, which I had observed in the vinegar before distillation. To separate this spirit from the water with which it was mixed, I threw into the liquid a large quantity of dry carbonate of potash. The water being saturated with this, the surface of the solution was covered with a very thin pellicle of this spirit; too little indeed for me to estimate its quantity, but enough to leave no doubt of its existence.

Spirit from it.

Mucilage, or extract,

The liquid, which was clear and colourless, as I observed, after distillation, and before I had thrown any carbonate of potash into it, was rendered turbid by the solution of this salt, and became of a violet brown. Some flocks remained suspended in the water. It is this matter which Mr. Darracq calls mucilage, but which, according to Mr. Steinacher, is extractive matter.

passed over twice in distillation.

It is to be observed, that this substance, whether mucilage or extract, had passed over in distillation twice: first when I distilled the original vinegar, and next when I redistilled it after the combination of its acid with potash. I found it

it very difficult to deprive vinegar of this entirely by repeated distillations.

In a similar way I examined French vinegar. That I used was of the specific gravity of 1·0072. The proportion of its acidity to that of the English vinegar was as 4·01 to 3·46. In general it contained less mucilage and more spirit than the English. I met with a vinegar in the shops at Paris, that contained a very sensible portion of alcohol. Four quarts distilled from carbonate of potash yield d two ounces of a very light fluid, from which I separated 0·40 of ardent spirit. French vinegar.

From these experiments it appears, that the vinegar I employed was composed at least of water, acetic acid, vegetable matter, and a small portion of a spirituous liquor. Component parts.

I distilled 4lbs of acetate of copper, dividing the product into five nearly equal parts. Each of these I rectified by a second distillation to dryness. The first portion had the specific gravity of 1·0659 the third of 1·0580, the fourth of 1·0434, the fifth of 1·0100. An accident prevented my examining the second. On saturating these portions with the same base, I found the quantities of acid contained in them to be in the following proportion, 62·971 for the first; 67·461 for the third, 74·111 for the fourth, and 73·295 for the fifth. Acetate of copper distilled.

But this series, the last term excepted, is increasing, while that of the specific gravities is uniformly the reverse.

Messrs Derosne have just published a paper on this subject; but when I observed the same facts in 1803, I found from the scientific collections of the last century, that Courtenvaux, Monnet, and Lassonne had noticed them fifty years ago. These facts, lately noticed by Messrs Derosne, observed long ago.

In the year 1754 the Marquis de Courtenvaux, examining the present subject, says, that the first portion that passes over on distilling acetate of copper is not inflammable; and that, though heavier than the subsequent portions, it is less acid. This perfectly agrees with what I have just said. In the same work he observes, that the last portion easily takes fire; and other chemists have made the same remark. The

 smell

smell of the last portions too is more pungent than that of the first. All these appearances depend on a spirituous very light, highly inflammable, and extremely pungent fluid, which forms at the close of the distillation of acetate of copper. Messrs. Derosne have given it the name of pyroacetic ether; but this seems to me to be determining its nature too precisely, and I have called it by the more general term of pyroacetic spirit.

Their pyroacetic ether more properly spirit.

No experiment I made indicated the presence of mucilage in the liquid product of the distillation of verdigrise. It appears to be composed at least of water, acetic acid, and pyroacetic spirit.

Difference between acetic acid and vinegar.

From these results we may appreciate the slight but real differences, that exist between vinegar and the product of the distillation of acetate of copper; and which had formerly led Berthollet and Chaptal to believe the existence of two acids.

In English vinegar, for instance, there is a little acetic acid, a little vegetable matter, and extremely little spirituous liquor. In the product of distilled acetate of copper there is more acid it is true, but no vegetable matter, and much more spirituous liquor. On account of the lightness of this liquor therefore, there ought to be more acetic acid in the product of the acetate of copper, than in vinegar of equal specific gravity. Accordingly Mr. Berthollet, having saturated equal quantities of these two fluids reduced to the same specific gravity, obtained more acetate from the former than from the latter: and Mr. Chaptal has observed, that vinegar required one sixth less of base than the product distilled from acetate of copper, under similar circumstances. I have composed liquids in which a quantity of acid was compensated by a portion of pyroacetic spirit, in order to keep the specific gravity the same as that of vinegar; and the quantity of base required to saturate each was exactly in the ratio of the spirituous liquor.

Vegetable matter.

The vegetable matter too accounts for vinegar being more highly coloured by the addition of sulphuric acid, than the product of acetate of copper. It accounts for the greater quantity of carbon in the destructive distillation of the acetate formed by the combination of vinegar with potash, than

than in that of the acetate obtained by the union of this alkali with the product of acetate of copper. This it is that obliges the druggists to expose what they call foliated earth of tartar to a moderate heat, in order to render it white. Lastly, when Mr. Berthollet imagined, that by means of acetic acid he had expelled acetous acid from its combination with potash, it was because we can in fact obtain an acid milder than the acetic, and nearer to the state of vinegar, by distilling acetic acid from an acetate of potash in which all the vegetable matter has not been destroyed by heat. Part of this, I believe, is carried off by the fresh acid, till the equilibrium of affinity between it and the salt in which it existed before is restored.

Purification of acetate of potash.

From these experiments it follows, that the specific gravity is not a faithful index of the quantity of acetic acid contained in vinegar, and in the product of the distillation of acetate of copper,* because neither of them is a pure and simple solution of acetic acid in water.

Spec. grav. no test of acidity.

To place all this in a more striking light, I availed myself of the researches of Lassonne and Monnet, who have instructed us, that the acetate of lead, or of zinc, distilled like that of copper, gives us a larger quantity of the spirituous liquor. Accordingly I subjected to distillation two pounds of acetate of lead, and collected the produce in three portions. The first was nothing but weak acetic acid; the second, rectified by distillation to dryness, was of the specific gravity of 0·9234; and the third, of 0·8567. Their acidity was in the ratio of 4·421 to 5·462, and the spirituous liquor as 60·50 to 83·25.

Acetate of lead, or of zinc yields more spirit than that of copper.

Comparing the various properties of lead and copper, and particularly their reducibleness, with the difference of the results in the distillation of their acetates, I was led to inquire into the action that takes place between some other metals and vegetable acids in similar circumstances, in hopes of connecting together the phenomena of the distillation of metallic acetates, and deducing from them a general law respecting the formation of the pyroacetic spirit.

Other metallic salts compared.

The resistance, that any salt opposes to the action of heat, is proportional, *cæteris paribus*, to the affinity of the acid

Resistance of a salt to heat, as

the affinity of its parts, unless one of these is destructible.

for its base. But if the acid, or base, be decomposable at a lower temperature than would separate them were it not for this destructibility, the affinity that unites them is a function of that on which their composition depends. The phosphate of lime does not lose its acid at a temperature, at which sulphate of lime parts with its acid, because the latter is decomposed.

Metallic oxides.

Let us take then for bases the metallic oxides; but let us first unite them with one particular acid, so that all the differences in the results of the decomposition of the salts they form may depend on the variation of one substance alone.

Circumstances to be attended to.

It is true, that each base is accompanied with fresh circumstances, independently of the facility with which it is decomposed. The proportions too of water, of base, and of acid, are not to be neglected.

State of the oxide before the process.

Another essential consideration is the state of the oxide before the process, and that to which it has a tendency during its taking place. Metallic silver, for example, is not soluble in any acid: manganese oxided at 0·66 is equally insoluble; but after adding oxigen to the one, and abstracting oxigen from the other, saline combinations of these metals may be formed. Chemists speak of the gray oxide of silver with 10 per cent of oxigen, and the white oxide of manganese with 20 per cent, as those of their respective metals that are most soluble. But this gray oxide of silver is reducible by a gentle heat, and the white oxide of manganese is liable to be superoxided by taking up 46 per cent more of oxigen. We must pay attention therefore to the 0·10 that one gives out, and the 0·40 that the other may absorb.

Acetates of silver and manganese

The acetates formed by these two oxides may be considered, in this point of view, as the extremes of a series, the intermediate terms of which are to be found among the other oxides. Let us then examine in detail the acetates of silver, copper, nickel, lead, iron, and manganese.

Composition of the salts.

To compound these salts in a uniform manner, I prepared the oxides of the different metals by the most appropriate means the art of chemistry teaches us; and I always satisfied myself of their purity, before I made use of them. I then

then combined them directly with distilled vinegar, or with pure acetic acid, according to the object I had in view.

The oxide of silver dissolves very well in vinegar, and in diluted acetic acid; and the salt it forms with both is so remarkable, that it would be sufficient to prove the identity of the two acids. On evaporating the solution, a pearly acicular salt is obtained, grayish if vinegar be employed, white if formed with the pure acid, very light, and very soft to the touch. If 100 parts of the mother water of these crystals at a heat of 15° [59° F.] be evaporated, about one part of salt will remain. If the mother water be hot, more will be obtained. This salt may be procured likewise by pouring a concentrated solution of an alkaline or earthy acetate into a solution of nitrate of silver, washing the precipitate, and crystallizing it afresh. Acetate of silver.

The acetate of copper is well known. I have sometimes operated on that of the shops; but in my experimental researches I formed it myself from its component parts. Acetate of copper.

The acetate of nickel I procured by directly combining acetic acid with the oxide of this metal purified by the new processes. It is sufficiently soluble, and crystallizes well. Its solution is a fine deep green; the crystals are of a somewhat lighter colour. Acetate of nickel.

The acetate of lead is at least as well known as that of copper. But it must be remembered, that it exists in two states; that in which it is found in the shops, and that described by Mr. Thenard. Either may be converted into the other, by adding oxide to the former, and acid to the latter. I believe there is a third state of this salt, intermediate between the two. Acetate of lead.

The acetate of zinc is very soluble. It crystallizes confusedly, and liquefies in its water of crystallization, the quantity of which is pretty considerable. Acetate of zinc.

If a solution of acetate of iron be left exposed to the open air, red oxide will separate, retaining some acetic acid. Crystals are obtained from it with great difficulty. Some have spoken of a spontaneous inflammation taking place on evaporating a solution of this salt: but I have never seen it, though I have evaporated acetates of iron at Acetate of iron.

least

least five or six times. I took no particular care, either to produce or to prevent it.

Acetate of manganese.

The acetate of manganese crystallizes better than the acetate of zinc. Rhomboidal laminæ may be distinguished among its crystals. Its colour resembles the smoky topaz, if it be formed with vinegar; but with acetic acid it is whiter. It contains less water of crystallization than the acetate of zinc, yet it liquefies at a high temperature.

Difficult to have the acetates perfectly free from water.

Whatever precautions I took to obtain these salts in a state of dryness, I cannot venture to say, that I have succeeded. Those that crystallize well, as the acetates of silver, copper, and nickel, may be considered as in a state nearly uniform. But the acetate of zinc retains a very large proportion of water in the mass of its confused crystals; and unless it be reduced to very fine particles, it remains in it, and occasions its liquefaction.

Real acetic acid not known.

I would have endeavoured to analyse these salts, if I had conceived any hope of doing it to my satisfaction: but I am not acquainted with the real acetic acid, and I believe it is unknown in chemistry. Some rule however is necessary, by which we may appreciate the state of the substances, on which I operated. The proportion of its weight that each of them loses at a heat capable of decomposing it entirely may serve us as a guide: and accordingly I exposed a known quantity of each to a high temperature in a platina crucible, weighed before and after the operation.

Action of heat on the acetates of silver,

The acetate of silver exposed to the flame of a candle alone gives out a strong smell of acetic acid, and is reduced almost without any change of appearance, so that it resembles native plume silver. It becomes very white, and does not retain carbon, like the other metallic acetates, unless the contact of air be excluded. In open vessels it loses 3·631 of its weight out of 10·000.

copper,

The total decomposition of the acetate of copper is perfectly accomplished in a glass vessel. Exposed in a capsule on a heated sandbath, it first decrepitated. A light vapour of acetic acid then manifested itself at a very low temperature; and the salt became brown round the sides of the vessel. This colour spread gradually to the middle, and

the

the copper acquired a fine metallic brilliancy throughout the whole mass. A pretty bright light then seemed to flash over its surface, and the metal lost its brilliancy, becoming like turnished copper. It is at this juncture, that the residuum of the distilled acetate of copper has been considered as a pyrophorus. The maximum shown by Fahrenheit's mercurial thermometer during the process was 417°. This experiment, carefully conducted in a platina crucible, in order to appreciate the loss on 10·000 parts, gave 6·441 dissipated by the fire. Some carbon remained with the metallic copper.

The acetate of nickel, exposed to an open fire, lets its acid escape. It becomes black, and we see flushes of light darting over its surface, as in the process with acetate of copper. It retains a little carbon after the operation, and loses 6·261 of 10·000. nickel,

Ten parts of acetate of lead of the shops swelled up before losing their acid. A greater heat is required to decompose this salt, than those I have mentioned; and the smell is not that of acetic acid. It becomes at first black, then yellow, then red, when heated in the open air; and its carbon burns as well as the metal. It loses 3·552. That of Mr. Thenard does not swell up. In other respects it exhibits the same appearances, but loses in the fire only 1·635. lead,

The acetate of zinc loses its acid, and becomes less black. It burns, and grows black. It loses 6·025; but as a part is volatilized, this experiment is less to be depended on than those with the other acetates. zinc,

The red mass of acetate of iron loses commonly about 4·500: but it is difficult to obtain it in a uniform state. iron, and

The acetate of manganese exhibits similar phenomena, and loses 7·186. manganese.

The proportions I shall now proceed to give relate only to the salts that lose in the fire the quantities abovementioned.

For the distillation of these salts, I introduced a known quantity of each into an earthen retort, or one of coated glass, according to the degree of heat necessary for the operation. To this I joined a tubulated matrass, and a Woulf's phial containing solution of barytes: and the pneumatochemical Distillation of these acetates.

matochemical trough terminated the apparatus. I began the operation with the lowest degree of heat possible, and always endeavoured to keep it at a minimum.

Results.

The results were three products, which it was necessary to examine. First there remained in the retort the metallic base; and to ascertain the state in which it was left by the acid, I submitted it to the docimastic experiments suited to each metal.

Residuum of the acetates of silver,

The residuum of distilled acetate of silver dissolved in nitric acid with evolution of nitrous gas. A black matter remained, which did not dissolve. This, when washed and dried, weighed 0·05, and was charcoal. The nitric solution produced the same quantity of muriate of silver as 0·95 of metallic silver.

copper,

Messrs. Adet and Darracq have said, that, after the distillation of acetate of copper, the metal is oxided, according to the former with 0·08 of oxigen; and according to the latter so as to be soluble in muriatic acid. Hitherto only two oxides of copper have been mentioned; one, which is brown, containing 0·20 of oxigen; the other red, containing 0·17 of oxigen according to Proust, but from experiments on the octaedral red copper ore from Cornwall I conceive it should be estimated at 0·115. I dissolved in nitric acid 10·000 of the residuum of the distillation of acetate of copper. I filtered the solution, and 0·055 remained on the filter, which had all the properties of carbon. The filtered solution I evaporated to dryness twice, adding muriatic acid each time. I then precipitated the copper by purified zinc, and obtained 9·4 of metallic copper. Now if the copper had not been in the metallic state in the residuum of the distilled acetate, I should not have obtained the original weight of the matter employed; and the deficiency would have indicated the quantity of oxigen. On another 10·000 parts I poured muriatic acid, carefully preventing the contact of air: and at the expiration of ten days not an atom of copper was dissolved. By reduction by fire, by alkalis, and other docimastic means, I satisfied myself, that the copper is truly in the metallic state in the residuum of the distillation of acetate of copper. Mr. Darracq found an insoluble residuum of 0·22 after the action

action of muriatic acid. In my process I found but 0·05 or 0·06, beyond which there was no sensible variation. His residuum must surely have contained copper, that had escaped the very imperfect action of the muriatic acid.

The distilled acetate of nickel leaves a black residuum. In muriatic acid 10·000 parts dissolved with a brisk evolution of hidrogen gas; and 0·14 of carbon remained. The nickel in this residuum was found to be in the metallic state; for it yielded the same quantity of precipitate by alkalis as 10·000 parts of nickel, deducting the carbon. *nickel,*

The residuum of the distillation of acetate of lead is in the metallic state. It leaves 0·04 of carbon; and precipitated from its solution in nitric acid by a sulphate, it yields the same quantity of sulphate of lead as an equal weight of metallic lead. Mr. Trommsdorf has said, that what is left in the retort after this distillation is oxide of lead: but I never found the lead in the process oxided but once, and that was when the retort had cracked. It is only when the acetate of lead is exposed to the contact of air during the distillation, that the metal is oxided, as I have already observed in speaking of the loss it undergoes when exposed to heat in open vessels. *and lead.*

The residuum of the distillation of all these acetates, which contain carbon, are more or less pyrophori. It is not at all strange, that charcoal when in a state of minute division should take fire more easily than when compact. *All these pyrophori.*

The residuum of distilled acetate of zinc dissolves in muriatic acid directly, and without effervescence; and 0·050 of charcoal remain. The zinc is in the state of white oxide, as it was in the salt previous to distillation. *Residuum of the acetates of zinc,*

After the distillation of acetate of iron, black oxide of iron remains, with 0·02 of carbon. It dissolves in muriatic acid, and yields the muriate formed by black oxide of iron. *iron, and*

The acetate of manganese leaves a brown residuum. When muriatic acid is poured on it, it dissolves, giving out oximuriatic acid, and leaving 0·035 of carbon. *manganese.*

We will now proceed to examine the liquid products. The matrass in which they were condensed was constantly surrounded with a frigorific mixture, as was its neck also, *Liquid products of the distillation.*

which

which I always chose very long. In consequence nothing escaped but permanently elastic fluids.

In these liquid products three things were to be ascertained: their specific gravity, their degree of acidity, and their proportion of spirituous liquor.

Method of finding specific gravity.

To find the specific gravity, I first took the weight of a small phial with a ground stopple, which at the temperature of 15° [59° F.] contained exactly 10 gram. [154·4 grs] of distilled water. The weight of any other fluid contained in this phial at the same temperature gave the specific gravity directly. This method, on which Mr. Descroizilles has published a very minute paper, is far from new; but he has not ascribed to it all the advantages it actually possesses. It merits the preference particularly because it avoids all friction from immersion in the liquor to be assayed, and has no limits but the sensibility of the balance employed. That I used was sensible to a thousandth of a gramme, which gave me the specific gravity sought to a ten thousandth nearly.

Objections answered.

To this method it has been objected, that the stopple may be thrust in more or less in different experiments, so as to force out more or less of the fluid. The dilatability and elasticity of glass too have been mentioned. But let any person repeat the experiment several times on the same fluid, and he will find, that the differences will only affect the thousandth or ten thousandth parts. This bottle of a known weight is equally adapted for taking the specific gravity of solid bodies.

Methods of finding their degree of acidity.

To find the proportion of acidity of these liquids, I had hoped, that by employing a substance, which by its own action on a solvent should be divided into a soluble and insoluble matter, I should be able to substitute a law of nature for the uncertainty of manipulations. Accordingly I tried carbonate of lime. But acetic acid does not attack this with sufficient vehemence, to attain the limit of its action in a uniform manner: and having put some carbonate of lime into a glass retort, in order to dissolve it in acetic acid, I collected the fluid that passed over by distillation, and found it required repeated cohobation, before it

it ceased to give signs of acidity; which rendered the process complicated, and deprived it of precision.

I had equally hoped, that carbonate of potash thrown into acetic acid would have given me the ratio of acidity by the quantity of carbonic acid evolved: but I found, that trials of this kind differed more from each other than the following.

That adopted.

I exposed potash to a strong red heat, yet I was far from supposing it to be totally deprived of water. For more facility of proceeding, and to reduce any errour to a tenth part, I dissolved one part of this potash in nine parts of distilled water. Into a given quantity of the liquids resulting from the distillation of the acetates I poured some of this solution; and when the tinged papers indicated that the point of saturation was at hand, by letting it fall drop by drop from a very slender tube I attained a degree of accuracy more than sufficient, to answer the other parts of these researches.

(To be concluded in our next.)

XIV.

On the Precipitation of a Solution of Sulphate of Lime by Sulphuric Acid. By T. LE GAY BREWERTON, *Fellow of the Royal Physical Society, Edinburgh.*

To Mr. NICHOLSON.

SIR,

IN a paper on the acids produced by treating ginger root with nitric acid, which you honoured with a place in your Journal*, was this note.

Sulphate of lime precipitated by sulphuric acid,

"The fact of sulphuric acid causing the deposition of "sulphate of lime from a state of solution, to me is not a "little surprising, however it may perhaps be well known "to those more versed in chemical experiments and in no "manner puzzling."

owing to impurity of the acid,

That sulphate of lime may be rendered more soluble by an excess of acid, is a well established fact; but that a still greater excess of acid should cause the deposition of the sulphate already dissolved, seems an operation of so anomalous a nature, that it cannot be received as true without the greatest scepticism. Impressed with this idea, I attempted to discover the source whence the deception had originated, and found it to be in the sulphuric acid.

The

* Vol. XXV, p. 177.

which contained sulphate of lime.

The sulphuric acid contained a quantity of sulphate of lime, which was deposited on diluting the acid, and which presented crystals precisely similar to those in the paper alluded to.

Necessary to examine the purity of reagents.

Though being deceived by the impurity of the sulphuric acid does not affect the general conclusions as to the acid products obtained by treating ginger root with nitric acid, yet it shows the crystallized matter in the experiments did not arise from using white lead, although it was adulterated with chalk, but from using impure sulphuric acid; and shows too the necessity of young experimentalists paying particular attention to the purity of the reagents employed.

I am, Sir, yours very respectfully,

Bawtry, June 13, 1810. T. L. G. BREWERTON,

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Mineralogy of the Highlands.

AT the meeting of this Society on the 7th of April, Dr. Macknight read a mineralogical notice, on the tract of the Highlands from Killin to Braemar, by the way of Glen Zilt. Ben Lawers is composed of undulated mica-slate, which at the summit is yellowish-gray, and in some varieties so full of quartz as to resemble a sandstone. Towards Lagierait, beautiful garnets begin to appear. Beyond Mullevearn, gneiss occurs; also limestone, hornblende-slate, and sienite. Besides the substances first mentioned, Glen Zilt is remarkable by a peculiar aggregate of feldspar, hornblende, and occasionally quartz; in which the various proportions of these ingredients exhibit the rock under various aspects of the sienitic and greenstone species. It is distinguished from granite (for which it has been mistaken) not only by the unerystallized state of the feldspar, but by the presence of hornblende, and the absence of mica. Professor Jameson has called it sienitic greenstone. It occurs in conformable beds; particularly one of great size, which intersects the channel of the river at different places, near the lodge. Crossing the mountains from Glen Zilt to the course of the Dee, we find hornstone, feldspar-porphyry, and limestone, subordinate to mica-slate and gneiss; till we reach the castletown of Braemar, where the granite of the Grampians at length appears.

At

Vertical conglomerate rock of the Grampians. At the same meeting, a communication from Col. Imrie was read, describing the conglomerate rock of the Grampians, and tracing it from near Stonehaven to the Burn, and again at Callender, eighty miles distant to the N. W. The position of this conglomerate rock is vertical; and of this fact, in Col. Imrie's opinion, no satisfactory explanation has yet been given.—At this meeting, also, there was laid before the Society an accurate section of the coal-field at Alloa, accompanied with interesting remarks, by Mr. Robert Bald, civil engineer, and manager of Mr. Erskine of Mar's extensive coalworks. The depth of the section is 704 feet; the alternating strata are 141 in number; and the total amount of the thickness of the different beds of coal, is 59 feet 4 inches. Captain Laskey likewise presented to the Society a cabinet containing a series of the remains of a fossil encrinus found in slate-clay near Dunbar.

Coal field at Alloa.

Fossil encrinus.

Mathematical Repository. Mr. Leybourn of the Royal Military College, has just published the tenth number of the Mathematical Repository, containing; solutions to the mathematical questions proposed in the Eighth number, and a series of new questions to be answered in a subsequent number; an Essay on Polygonal numbers; a new demonstration of the Binomial Theorem; an illustration of the forty-seventh proposition of the second book of the Principia; a curious indeterminate problem; solutions to a curious problem in Dynamics; and a continuation of Le Gendre's Memoir on Elliptic Transcendentals.

Treatise on Fluxions and the application to naval and military science. Mr. W. Moore, of the Royal Academy at Woolwich, has in a good state of forwardness a Treatise on the Doctrine of Fluxions, with its application to all the most useful parts of the true Theory of Gunnery, and other very important matters relating to Military and Naval Science. The fluxions will be preceded by such parts of the science of mechanics, as are necessary for reading the work without referring to other authors; and the whole will be so arranged, that any person moderately skilled in algebra, geometry, and trigonometry, and having a knowledge of the most common properties of the conic sections, may proceed to these inquiries without any difficulty. The whole will be printed in 1 vol. 8vo, and will be particularly adapted to all Military Institutions of eminence.

Meteoro-

METEOROLOGICAL JOURNAL,

For JUNE, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

MAY Day of	Thermometer. 8 A. M.	Noon.	11 P. M.	BAROMETER, 9 A. M.	WEATHER.
28	48°	56°	45°	30·06	Fair
29	46	51	46	30·38	Ditto
30	49	65	51	30·36	Ditto
31	52	66	50	30·32	Ditto
JUNE.					
1	53	65	52	30·29	Fair
2	54	69	54	30·25	Ditto
3	53	66	50	30·24	Ditto
4	52	65	55	30·26	Ditto
5	54	55	50	30·24	Cloudy
6	51	70	55	30·24	Fair
7	54	67	56	30·16	Ditto
8	56	70	61	30·12	Ditto
9	59	73	55	30·00	Ditto
10	59	70	55	29·73	Cloudy
11	58	68	54	29·80	Fair
12	54	66	53	29·95	Cloudy
13	55	60	50	29·90	Rain
14	50	63	54	30·10	Fair
15	54	66	51	30·24	Ditto
16	52	62	46	30·09	Ditto
17	50	60	54	29·98	Ditto
18	56	70	60	29·94	Ditto
19	60	70	60	29·98	Cloudy
20	63	72	63	29·96	Ditto
21	64	77	66	30·18	Fair
22	66	73	55	30·30	Cloudy
23	54	69	55	30·40	Fair
24	56	73	60	30·23	Ditto
25	59	73	66	30·16	Ditto
26	59	61	58	30·09	Cloudy

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1810.

ARTICLE I.

Analysis of the Galvanic Pile. By J. A. De Luc, *Esq. F.R.S.*

PART II.

Pile separated by writing paper.

I Concluded the first part of this *Analysis* by an experiment, in which a pile of 76 groups of *zinc* and *silver*, the number used in my preceding experiments, separated by pieces of *writing paper* substituted for the *wet cloth*, though giving very sensible *electric* signs at its extremities, produced no *chemical* effects in the water of the usual glass tubes applied to it, nor the *shock*.

Difference between the electrical & chemical effects of the pile.

This experiment was made during my investigation respecting the causes of the two distinct effects of the *pile*, namely *electric* and *chemical*, which by the foregoing experiments had been proved to be different; the former depending only on the *binary groups* of the *metals*, separated by the best *conducting* substance *nonmetallic*, and thus not necessarily productive of *chemical* effects; these requiring absolutely, that the interposed *nonmetallic* substance be *wet*; a circumstance from which the *chemical* effects depend on

groups composed of the *two metals* with the *wet body* between them. Such is the fundamental proposition, with respect to the *mode of action* of the *galvanic pile*, which was to be submitted to farther experiments.

Conducting faculty of organized substances owing to moisture.

The *conducting* faculty of all the substances of the *vegetable* and *animal* kinds, that may be employed to separate the *binal groups* of *metals*, for transmitting from one to another their individual *electric* effects, mostly depends on a certain quantity of *moisture*, with which they are usually penetrated. Of this I had soon an opportunity of being informed, by an experiment of my friend Dr. Lind, who had constantly followed the progress of these researches: he had constructed a pile similar to mine, except that *copper* plates were substituted for *silver* plates, which acted also immediately upon the gold-leaf electroscope; but he found the following change in it.

Exp. 21. The paper highly dried. The action diminished.

Exp. 21. He dismounted this pile, and laid all its parts on his hearth, before a great fire, so that the pieces of paper were almost singed, and mounting it again in this state, it did not sensibly affect his electroscope: he brought it into my room, and it affected only the electroscope of my condenser. We then dismounted the pile, and laid all its parts on a table for one hour, my hygrometer being about 40°, and when again mounted, it acted on the electroscope as it did before the *papers* had been so thoroughly *dried* by a great heat.

Water not an insulating body for low intensities.

This experiment shows, that Mr. Davy was mistaken when he thought, "that with respect to *electricities* of such "*low* intensities, *water* is an *insulating* body." On which principle he considered the *mode of action* of the *pile* under the idea of what he calls *induction*; meaning, I suppose, something similar to the effect of Volta's condenser, when it operates by the interposition of *silk* between the *metallic* plates. If this were the case, pieces of *silk*, through which what he calls *induction* is readily produced, being placed between the binary groups of *metals*, should produce a still greater effect than *paper* in its common state, as more *insulating*; whereas the foregoing experiment proves, that the effect almost ceases, when *paper*, by thorough *dryness*, is reduced to the state of an *insulating* body. I insist on this point

point only, because it involves one of the most important questions in terrestrial physics. Must we, *in the present state of our knowledge*, be satisfied with *electrical energies*, which might be considered as *essential properties of matter**? Or rather, in this very *state of our knowledge*, is it not already ascertained, that a particular *substance* exists, namely, the *electric fluid*, which, beside the effects here in view, produces greater and more general effects on our globe? This question is the main object of this and the following papers, and I hope it will be decided by facts.

Electricity not a quality of matter, but the effect of a distinct fluid.

Having found no *conducting* substance more convenient than *paper*, for transmitting from one to another of the binary groups of *metals* their individual effects, I came to consider whether it might not be of some advantage, on account of the very small quantity of *electric fluid* thus set in motion, to produce a closer contact of the *paper* with the *metals* between their groups. I made various experiments, by pasting the *paper*, first on both *metals*, then on one only, in the intervals where I had placed it loose. My condenser rendered easy these trials, as I could make them on piles of only 20 groups, and the following was the general result. There is an increase in the *transmission* of the *electric fluid*, when the *paper* is pasted on the outside of such group, upon the *metal*, which becomes *negative*; but the reverse takes place, when it is pasted upon the *metal* which becomes *positive*.

The transmission of the electric fluid promoted by pasting the paper to the negative metal, & *vice versa*.

In the following experiments each pile of 20 groups was placed upon the moving metallic pillar of my condenser, and raised into contact with the receiving plate of the latter, where I left it during 20 seconds; then, letting it down, and removing the upper plate, I observed the divergence of the gold leaves, and noted it in *decimals* of an inch, as in the experiments of the 1st *part*. In these operations, the end of the small piles, which rested on the pillar communicating with the ground, was thereby *neutral*; and the whole *electric difference* between its extremities was expressed, with its proper *sign*, by the electroscope of the conden-

Mode of conducting the experiments to prove this.

* Mr. Davy's *Bakerian Lecture*, Ph. Trans. 1807, Part I, p. 39: or Journal, vol. XIX, p. 50.

 ser.

ser. The first experiment on each association of *metals* was made with the papers *loose* between the *groups*, in order to observe afterward the differences resulting from the *pasting* them on one or the other of the *metals.*

Exp. 22. *Exp.* 22. With 20 groups *zinc* and *silver*, separated by *loose* pieces of *paper.*

Silver side on the *pillar*, *zinc* side applied to the *cond.* *pos.* 0·35.
Zinc side .... *silver* side .... *neg.* 0·35.

The *difference* between the *extremities* being commonly equal, with only opposite *signs*, when these *extremities* are in turn applied to the condenser, I shall suppress the *sign* when the *quantity* only of the *difference* will be the object of consideration.

Exp. 23. *Exp.* 23. This will give the first proof of the advantage of pasting the *paper* on the *metal* which becomes *negative* in contact with the other; this being the case of *silver* in the above pile.

20 groups *zinc* and *silver* separated by *loose* paper....0·35
20 groups............. paper *pasted* on the *silver*..*the gold leaf struck the side.*

Zinc and *silver* (or *copper*) are, as yet, the *metals* which, being associated, differ the most in their *electric* states, and for this reason they are commonly used in the pile; but in order to ascertain whether the advantage manifested by the above experiment, of pasting the *paper* on the *metal* which becomes *negative* in each group, was general, or only a circumstance attached to *silver* and zinc, I thought of employing some *metal*, which became *negative* with *zinc*, but *positive* with *silver*; I found *pewter* such an intermediate metal, from the following experiment.

Exp. 24. Pewter neg. with zinc, pos. with silver. *Exp.* 24. With two *piles*, each of 20 groups, the first *zinc* and *pewter*, the second *pewter* and *silver*; the groups separated by *loose* pieces of *paper*; each *pile* being alternately placed in communication with the *ground* by one side, while the other communicated with the *condenser.*

1st pile

1st pile { *zinc* side applied to the condenser .... *pos.* 0·10
{ *pewter* side .... *neg.* 0·10

2d pile { *pewter* side applied to the condenser .. *pos.* 0·20
{ *silver* side .... *neg.* 0·25

This experiment first affords a new proof, that *negative* and *positive* are no *qualities* attached to certain *metals*, since here *pewter* is *negative* in its connexion with *zinc*, and *positive* when connected with *silver*. It is only an accidental circumstance, that there is as yet no known substance with which *zinc* becomes *negative*, and none with which *silver* (or *copper*) becomes *positive*; and they have in themselves no tendency to either of these states, acquiring them, and that inversely, by particular circumstances, as will be seen hereafter. These qualities not inherent in certain metals.

With respect to my present purpose, this experiment having shown me in *pewter* a metal proper for the trials concerning the pasting of the *papers*, I made the following experiments.

Exp. 25. With 3 piles, each of 20 groups *zinc* and *pewter*. Exp. 25

1. The *paper pasted* on *zinc* .... almost no effect.
2. .... *loose* between the *groups* .... 0·1
3. .... *pasted* on *pewter* .... 0·5

Exp. 26. With 3 piles, each of 20 groups *pewter* and *silver*.

1. The paper *pasted* on *pewter* .... 0·05
2. .... *loose* between the *groups* .... 0·2
3. .... *pasted* on *silver* .... 0·4

Though there is not so much increase of effect in the last, as in the two preceding experiments, it shows still the same general circumstance, that there is some advantage, for increasing the transmission of the individual effects of each *group of metal* to the extremities of the *pile*, in fixing the *nonmetallic conducting substance*, on the outside of each *group*, upon the *metal* which becomes *negative* in its connexion with the other; a circumstance which led me soon to further steps: for these experiments presented me first the prospect of a *spontaneous*, as well as lasting *electric machine*, the shows, that the conducting substance should be fixed to the negative metal. Hint of a spontaneous electric machine.

the power of which could be increased by a greater number of *groups*.

Dutch gilt paper.

It would have been too expensive to proceed in that attempt with *silver* plates, and I was going to undertake it with *copper* plates, pasting the *paper* over them, when fortunately I thought of a *paper*, called *Dutch gilt paper*, on which a kind of *copper* is ready laid. At first I did not find the paper of that sort which is plain, but having tried that which is flowered, and finding much effect, I made the following experiment.

Exp. 27. Pile of this paper and zinc.

Exp. 27. I cut 76 pieces of this *paper* to the size of my *zinc* plates, 1·6 inch diameter, and having formed a pile of 76 groups, with only the *zinc* plates separated by the pieces of this paper, the *copper* side of all of them turned the same way, I found more *electric* effects at its extremities, than with the former pile of the same number of groups of *zinc*, *silver*, and *wet cloth*.

This experiment increased my hope of obtaining a *natural electric machine*, not indeed of a great power, but new in experimental philosophy, and which might lead to some farther discovery; but unluckily for speed, being sedentary at Windsor, I was not yet informed that, by a mode of rendering *zinc* ductile, thin plates could be obtained of it, which would answer my purpose, since in this use, calcination was not to be apprehended. Wishing however to proceed some way in this attempt, and having found, by trial with my condenser, that *tinned iron* plates had sensibly the same effect as *pewter* plates, I procured as many plates of the former, 1·6 inch diameter, as would fill up the two columns of my frame, separated as above by pieces of flowered *Dutch gilt paper*. There were 400 in each column, thus composing a pile of 800 groups.

Tinned iron plates & Dutch gilt paper.

Exp. 28. This pile gave strong electric but no chemical effects.

Exp. 28. The first object I shall mention of this experiment strongly corroborates the former conclusions concerning the *mode of action* of the *Galvanic pile*. Though this pile had, at its extremities, greater *electric* effects, than I have ever seen at those of any pile producing the strongest *chemical* effects in the *glass tubes* with *water*; yet these *tubes* being applied, there was not the least appearance of such effects.

But

But at the same time a new scene was opened to my view. This natural *electric machine* soon gave me reason to hope, that it might become a new and very essential *meteorological* instrument. Having no motive to dismount it, I left it a long time in the same state, observing the *electroscopes*, and the following are the general phænomena which they offered to me.

Hint of a meteorological instrument.

The pile left a long time,

1. The quantity of *electric fluid* put in motion by this pile was too great for the gold leaves to remain with a simple *divergence*; one of them *struck* the tin foil on its side, then fell, and *struck* again; which effect became the object of observation.

the gold leaf of the electrometer continued striking,

2. At times, these *strikings* were at both extremities of the pile, alternately. When the gold leaf *struck* on one side, it placed this extremity for an instant in communication with the ground; the gold leaf fell, but this communication with the ground increased the opposite electric state at the other extremity, where one of the gold leaves *struck* and fell; and this continued by turns.

sometimes at both ends alternately,

3. At other times the *strikings* were only on one side, and continued perhaps one day or two: then that state changed, and the *strikings* were at the other side.

sometimes at one only,

4. At which ever side the *strikings* were going on, there was a great difference in their *frequency*: sometimes the gold leaf seemed to beat *seconds*; while at other times it *struck* but once in a minute, or a longer time.

differing in frequency,

5. When the *frequency* of the *strikings* was upon the whole nearly equal many consecutive days, it increased successively from the morning till some time in the afternoon, and then went on diminishing till night.

which increased in the morning.

6. Sometimes I connected one side of the pile with the *ground*, by a metallic rod, which made the gold leaves fall on this, and increased the *frequency* of the *strikings* on the opposite side; but this increase was neither always the same, nor equal in the same time, when the communication with the *ground* was changed from one side to the other.

One end of the pile connected with the ground.

7. The beginning of my observations of this kind was in the winter of 1808; and in this season the *strikings* were upon the whole very frequent; but as the spring advanced, and perhaps as vegetation increased on the ground, there

Strikings more frequent in winter.

was

was a gradual diminution in the motion of the *electroscopes*, and a change in the effects on the pile from the communication of its extremities with the ground.

Unconnected with heat, moisture, or density of the atmosphere.

8. Lastly. These changes could not be attributed to those of either *heat* or *moisture*, at least in my room, and as influencing directly the pile; for the room remained nearly at an equal temperature, and there were but small changes in my hygrometer. I also frequently observed the barometer, and found no connexion between its variations and these great changes in the *strikings* of the gold leaves.

Owing to electricity.

From the whole together of these observations, I could attribute the variations in the *electric* phenomena of the pile only to changes in the *electric* state of the *ambient air*; or those in the effects of the communication with the *ground*, to changeable differences between the *electric state* of the latter and that of the *air*; an object on which we owe to Sig. Volta a great and fundamental truth. This justly celebrated experimental philosopher has made it evident, that *air* possesses the *electric fluid* as well as all the bodies which it surrounds; and on this principle he has founded the only real explanation of the *electric motions* of free bodies, such as the pairs of balls and the gold leaves of our electroscopes.

Volta showed, that the air contains the electric fluid.

The *electric fluid* tends to an *equilibrium* among all bodies, including the *air*, and there is a mutual attraction between it and all of them, weak with *conductors*, but strong with *nonconductors*, among which is *air*. The proportional quantity of *electric fluid*, which is *actually* possessed by *air*, is the *standard* of *plus* and *minus* in the *electroscopes*; and the proportional quantity of this *fluid*, which is *actually* possessed by the *ground*, is the *standard* of *plus* and *minus* concerning the differences in the *electric state* of insulated bodies compared with its own: these *standards* are sometimes similar, but they are often different. I shall not enlarge here on this beautiful theory, which I have fully explained in my works, and demonstrated by direct experiments*; but applying it to the pile, the immediate effects of the *ambient air* must be, to give some *electric fluid* to its *negative* side, which has proportionally *less* than itself, and to take some from the

Standards of plus & minus.

Effect on the pile.

* *Idées sur la Météorologie*, published in London by Elmsley in 1786 and 1787.

positive

positive side, which has *more*; and as this influence must change according to the changes in the *electric state* of the *air*,* the modifications of the *electroscopes* at the extremities of the pile may lead to unfold the latter, but not without farther discoveries.

Difficulties.

I was stopped in the progress of these researches by an incident, which has occasioned me much labour, and still retards them. This new *pile* is certainly in itself a *meteorological* instrument of great importance, as may be already judged, and shall be farther explained in the following paper; but in the state above described it was not fit for regular observations, and till the present moment * I have not yet surmounted all the difficulties: I foresaw them, and it was the reason why I wished, that the beginning of this new career should be soon known to experimental philosophers through the *Phil. Transactions*; in order that it might be followed by others, and probably with more success than by myself; but I am reduced to give the history of my own progress.

The gold leaves at last stuck.

From what has been above explained, the *strikings* of the *gold leaves* in the electroscopes were become the object of observation: for this purpose therefore they ought to have regularly continued; however, after one of the *gold leaves*, at either side, had alternately *struck* and *fallen* for some time, it at last *stuck* to the tin foil. The side therefore, to which this happened, was placed in permanent communication with the *ground*, which made the *strikings* to begin at the opposite side; but there also the gold leaf *stuck*. These adhesions continued, till, by a stroke on the top of the electroscope, the gold leaf fell; the oscillations were then renewed, but again stopped in the same manner. I tried various methods for preventing this adhesion, especially by substituting for the tin foil, which has a rough surface, polished laminæ of many sorts and forms, keeping them even at some distance from the glass, which might contribute to this effect; but all was to no purpose, still the gold leaf would *stick*. This was a great disappointment, and the only method I could devise was, to increase the power of a

Attempts to prevent this.

* Two years have elapsed since I communicated to the Royal Society the Experiments and Observations which are here my object.

pile

pile, till it could move small metallic balls, in hope that these would not *stick*.

Increase of the power of the pile.

Two ways presented themselves to my mind for this increase of power in a pile; one by using *plates* of a larger *size*, the other by increasing the *number* of the *groups*. The first method would have been cumbersome: but for a reason which I shall explain, I did not expect, that it would have the desired effect. However, even for the verification of my conjecture, I made the following experiment.

Exp. 29. Plates of different shape and size.

Exp. 29. I procured 10 *tinned iron* plates 4 inches square; 10 others round, of only 0·5 inch diameter: and I took 10 of my plates of 1·6 inch diameter. I cut pieces of *Dutch gilt paper*, the size of each of them, and compared their respective effects on my condenser. The result was beyond my expectation; I had only conjectured, that the increase of *size* would not increase the *divergence* of the gold leaves; now this was not only verified, but the largest plates produced the smallest effect. However, this unexpected difference probably proceeded from some accidental cause, which I had no time to investigate, and I considered this experiment, which I have often repeated under various forms, only as ascertaining the following proposition: that for the *electric motions*, considered solely with respect to the quantity of *divergence* in the *electroscope*, the *size* of the *plates* is indifferent; though, for the *frequency* of the *strikings* of the little pendula, and the intensity of the effects when the extremities of the pile are connected together, with the same *number* of *groups*, these effects increase with the *size* of the *plates*. This distinction, to which I shall return in the following paper, constitutes a part of the theory, which, as it made me foresee the result of the above experiment, I shall now explain.

Size indifferent with respect to divergence; but the frequency of the strikings increase with the size.

Volta's condenser does not show slight deviations of electricity in small bodies.

I am indebted for the ground of this theory to Sig. Volta; who, when in 1782 he showed me, at Paris, his then new-invented admirable *condenser*, explained to me, that it could not serve to manifest minute degrees of *deviation* from the *electric standard*, when belonging to *small* bodies but only to bodies of such an *extent* or *nature*, that the application of the *condenser* (by this taking its share of that *deviation*) does not sensibly lessen it. As an example of the

the first case, he gave me the atmosphere; and with respect to the *nature* of bodies, he took a *Leyden vial* discharged without a continued contact, the *residuum* of which, from its *nature*, may affect a moderate sized *condenser* without being much lessened: and to show me the necessity of this condition, he made use of the following analogy. When a piece of ground, by being swampy, indicates some stagnant water, if a *well* be dug there, the water will not fill it up to the level of the *stratum* of earth whence it proceeds, unless this *stratum* be of such an *extent*, that the quantity of water which gathers in the *well* has no sensible proportion with that contained in the stratum; so that the subtraction of this quantity cannot affect the *level* at which the water stands in the *stratum*. We have also an example of this case in the subject of electricity: when an insulated *electrified* body is small, we cannot know its real degree of electrification by applying a common *electroscope*; because this, sharing the *deviation* of the state of the body from the *electric standard*, lessens it too much for expressing what it was before that application.

Explained by analogy,

and by the common electroscope,

I shall use the former of these examples in explaining my theory concerning the difference of effects of the *size* of the *plates*, and the *number* of the *groups*, according to the use of the *pile*; and this explanation will chiefly consist in fixing the points of *analogy* between the two objects.

Theory explained.

1. I compare the *number* of *groups* in the *pile* to the *elevation* of a *stratum* whence water issues into a *well*.

2. The *size* of the *plates*, to the *extent* of this *stratum*.

3. The *degree* of *divergence* in the *electroscopes* at the extremities of the *pile* to the *level* which the *water* can attain in the *well* without *overflowing*.

These first *analogies* are sufficient to explain the case of the last experiment. When we attend to what is directly expressed by our *electroscopes*, we certainly do not expect, that this instrument shall indicate the *quantity* of *electric fluid* possessed by the bodies to which it is applied; for this would require also to measure their surface; we expect only to know the comparative *density* of the *electric fluid* among bodies, or its power to produce certain degrees of

Last experiment explained.

divergence

divergence in the *electroscope* we use*. Such therefore are the indications of the *electroscopes* at the extremities of the pile: they express certain *degrees* of *density* of the *electric fluid* on them, which are the same whatever be the *area* of the plates; these *degrees* depending only on the *number* of the *groups*, because each *group* contributes to increase the *density* of the fluid on one of the extremities by lessening it on the other. Thus it is that the *divergence*, both in *plus* and *minus*, of the electroscopes, at the extremities of the pile, is proportional only to the *number* of *groups*; in the same manner that the *height* at which *water* stands

Reasons for saying electroscope.

* I shall take this opportunity of explaining why I use the word *electroscope*, and not that of *electrometer*; it is because there is no instrument entitled to the latter denomination, at least admitted among experimental philosophers. Indeed, of our instruments serving to measure the degrees of intensity of physical causes, I know none *absolute*, except *hygrometers*, such as have been constructed by Mr. de Saussure and myself; for though these instruments are made of different substances, and differ in some other respects, we have obtained in both an absolute *zero*, and absolute maxima in determined cases, as well as determined degrees of intensity, of their object, namely *moisture*: all the other instruments intended for the same purpose, to my knowledge, are only *hygroscopes*, indicating *variations* in *moisture*, without determined *points*, or *degrees* common to them. Thus no kind of *physical instrument* has yet obtained the conditions of an absolute measure, but the above hygrometers, so little thought of by experimental philosophers, though very important in meteorology. The *thermometer* has obtained two *fixed degrees* of *heat*, and determined *divisions* of the interval between them, by which means experimental philosophers understand one another when they indicate certain *degrees* of *heat*. I have also constructed an *electrometer*, which possesses the same conditions with respect to *degrees* of *electrification*, which is described in my work, *Idées sur la Météorologie*; but not having been attended to by experimental philosophers, I have not been induced to follow the extension of this *measure* down to the minute degrees of intensity indicated by the gold leaf *electroscope*, as I could not expect that it should be more noticed: therefore admirable as is this instrument for its sensibility, it affords us no *comparable measure*. In this imperfect state, however, there is, in every *electroscope*, a property which belongs to no other *physical measure*, namely, a *natural* and *absolute* standard of *plus* and *minus*, which is *constant*, as to its general determination, and is the *actual* electric state of the *ambient air*, or the *ground*; though *variable* as to the absolute quantity, as are these electric states; which difference will be one of the objects of this paper.

Hygroscopes and hygrometers.

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in a *well* is proportional only to the *elevation* of the *stratum* whence it proceeds.

4. But when, in order to produce a *current* of water, a pipe is placed, or a trench is cut, on the side of a well, below the *level* at which the water stood in it, the *current* will be greater and more permanent, in proportion to the *extent* of the *stratum*, of the same *elevation*, whence the water proceeds; and also, when the *divergence* of the little *pendula* of the electroscope exceeds the extent that it can have without one of them *striking* the side, then *falling* by a momentary contact with the tin foil, which communicates with the ground, it will *sooner* rise and *strike* again, with the same *number* of *groups*, in proportion to the *size* of the *plates*; which last circumstance increases also the *current* of *electric fluid* circulating in a *pile*, the extremities of which are connected together by a conductor. Theory explained.

5. The *water* of all *springs* has the same source, namely, the *rain water* percolating through the ground, and retained on some impervious *stratum*, either argillaceous or stony. If this *water* do not find in its way any substance, with which it can combine, it comes out as it had fallen on the ground: but if in its course it combines with any substance, it may come out with certain *chemical* properties, different according to the substances which have combined with it. The case is the same with respect to the *electric fluid*, which pervades the *pile*; its source is no other than the *electric fluid* diffused over all terrestrial bodies, therefore over the *pile* itself. However we should be ignorant of the constant existence of this *fluid* over us and around us, were it not that, by artificial or natural operations, its *density* may be either increased or diminished on *insulated* bodies: this is the only circumstance which makes it appear, and that by the *electroscope* alone; for as long as this *fluid* remains in a state of equal diffusion over all bodies, it is manifested by no effect hitherto discovered. The *friction* between two bodies disturbs that *equilibrium*, in a manner which I shall show in a future paper on the *Analysis of the Electric machine*. But in the *pile*, which is my present object, it is by a property of its composition, that the *equilibrium* of the *electric fluid* is disturbed, whence proceed either the motions of the *electroscopes*,

scopes, or a *circulation* of the *fluid* through the *pile*, when the extremities of the latter are connected together by a *conductor*. Now, in the last of these cases, if the *electric fluid*, in its course, meets with no substance that changes its state; as is the case in a pile composed of *tinned iron*, or *zinc* plates, separated by *Dutch gilt paper*; we are indeed informed by the *electroscopes* of its *accumulation* on one extremity of the pile, and its deficiency on the other; however, neither *chemical* effects in the *circuit*, nor the *shock*, are produced; because the *fluid* remains unaltered: but when it pervades a *pile*, wherein, by a *liquid* being placed between the two *metals*, there is *calcination* of one or both of the latter, new effects appear: if the *liquid* be *pure water*, *chemical effects* are produced in the *circuit*, but there is no *shock*; if it be an *acid*, both effects are produced.

This theory laid before the Roy. Soc. in May, 1808.

These experiments, especially on the different effects of the *number* of the *groups*, and of the *size* of the *plates*, with the above theory on the cause of their different effects, were contained in my paper delivered to the Royal Society the 30th of May, 1808, about one month before Mr. J. G. Children executed in presence of Mr. Davy and Mr. Allen the grand experiment of the same kind related in *part* I of the *Ph. Trans.* for 1809*, by which the theory which I had already announced, was confirmed.

Questions on the mode of action of the pile.

But here two questions arise, which go deeper into the *mode of action* of the *Galvanic pile*, and they are these: 1. Of what *nature* is the *modification* produced in the *electric fluid*, when it pervades a *pile* wherein the *calcination* of some *metal* is going on? 2. What is the *cause* of the *motion* of this *fluid* in the pile, whether producing, or not producing the *shock* and *chemical* effects in the *circuit*?

The nature of the electric fluid answers the first.

The solution of the former of these questions, which leads to that of the latter, depends on the *nature* of the *electric fluid*; a subject much too long to be treated here; but it is fully detailed in both the works I have already referred to†; I shall therefore here confine myself to the conclusions contained in these works, as deduced from uninterrupted series of

* See Journal, vol. XXIV, p 150.

† *Idées sur la Météorologie*, and *Traité élémentaire sur le Fluide électrogalvanique*.

of experiments, of which I shall only describe the part necessary to my subject.

None of the phenomena observed in our common electrical experiments, namely, the *charge* and *discharge* of the *Leyden vial*, the *electric motions*, the effects of the *electrophorus* and of the *condenser*, had been really explained, till the inventor of the last two instruments, sig. Volta, had formed his theory on the *electric influences*, which threw the first true light on the modifications of the *electric fluid*; and which, in the course of various experiments I made to follow it through all the electric phenomena, gave rise to the system on the *nature* of the *electric fluid*, which I shall here briefly state. None of its phenomena really explained before Volta.

This *fluid*, far from being a *simple* substance, is an astonishing *compound*: and first, in its state which may be called *natural*, that, I mean, in which it is diffused over all bodies, it is found composed of two main parts, from which all the above mentioned phænomena arise. One of these two constituent ingredients of the *electric fluid* in this state is a substance, which, by itself, is not *expansible* (as in *steam*, also an *expansible fluid*, there is a substance which is not *expansible* by itself, namely *water*); this substance in the *electric fluid* I have called *electric matter*; and its function, which I shall soon point out, is very distinct. The other ingredient is an excessively subtile *fluid*, which (as *fire* in *steam*) uniting with the *nonexpansible* substance, produces the *expansibility* of the aggregate. In my French works I have called the latter *fluide déférent*; but here I shall call it *vector*, a short word of the same import, signifying that it *carries along* the *electric matter* (as, in steam, *fire* is the *vector* of *water*). Nature of the electric fluid.

The electric *vector* instantly pervades all bodies, and carries the *electric matter* through *conductors*, but not through *nonconductors*, such as *glass* and *resinous substances*: when a current of *electric fluid* arrives on one side of a *lamina* of these substances, and its *vector*, in order to establish its own *equilibrium* beyond it, pervades the *lamina*, it deposites the *electric matter* on the surface of the latter, where it remains adherent, till a current of *vector* pervades the *lamina* in the opposite direction; or it is taken up slowly by the *vector* in

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the air (as *fire* in *steam*, when it pervades a glass *lamina* to establish its own *equilibrium* beyond it, deposits the *water* on the side which receives the *steam*, where it remains, till it is carried away, either by fire coming from without, or by that spread in the *air*).

I come to the peculiar function of the *electric matter* in the above indicated pheuomena: it is the sole cause of *electric motions*, resulting from a *greater* or *less* proportional quantity of it, than is possessed by the *ambient air*; to which subject I shall return: the *vector* has no share in these *motions*, but as the *vehicle* of the *electric matter* acting in their phenomena. (As, with respect to *steam*, it is only *water* that produces the *hygroscopic* phenomena, without any interference of *fire*, except as the vehicle of *water*.)

By this system of a first *composition* of the *electric fluid*, the phenomena, which I have introduced in the beginning, are clearly explained in all their modifications, as I have abundantly proved by direct experiments in my works. But as long as the *electric fluid* remains in what I have called its *natural* state, moving along *conductors* and fixed on *nonconductors*, it produces no *chemical* effect hitherto known. What then does happen, when it produces these phenomena?

The electric fluid in its natural state produces no chemical effects.

How these effects are produced.

If we attend to this change, we shall observe a circumstance *sine qua non*, which is to contain some *cause*; it is, that the *conductor*, along which the *electric fluid* moves, *must* be *interrupted*. Now, when in this case the *electric fluid* darts through the *air*, three new phenomena are observed, *lucidity*, *heat*, and a particular *odour*. This cannot but indicate the *decomposition* of some particles of the fluid, occasioned by an excess of density, from which *light*, *fire*, and an *odorate* substance are disengaged: as when *steam* (to which from the beginning I have compared by analogy this system on the nature of the *electric fluid*) becomes too *dense* for the actual *temperature*, some of its particles, being *decomposed*, emit *water* and *fire*.

These new substances, *light*, *fire*, and an *odorate* substance, thus manifested in the composition of the *electric fluid*, are neither the *electric matter*, nor the *vector*, themselves, but must be contained in them, combined with some other

other substances, which prevent them from exercising their characteristic effects; a case most common in chemical compounds. The characteristic effect of *fire* is *heat*; when *free* it acts upon the *thermometer*; but it does not, when combined with other substances. *Lucidity* is the characteristic effect of *light*; but this is not *lucid* in phosphori, till they are *decomposing*: and also various bodies, while *decomposing*, emit *odorate* substances, which in their compound state had no *odour*. Now, the *light* emitted by the *electric fluid* probably belongs to the *vector*, which has many properties of the former; but it is not *lucid*, therefore *light* must be combined in it with some other ingredient. The *odorate* substance appears to belong to the *electric matter*, but this has no *odour*, therefore the former must also be combined in it with some other substance. Lastly, the *fire* emitted cannot be referred directly to either the *vector* or the *electric matter*; but probably, during their common *decomposition*, it is itself composed of the *light* and *igneous matter* disengaged. That *fire* is a *compound*, is a system which I have also treated with many experimental details in the above mentioned works.

Fire a compound.

No natural philosopher, who has applied to the study of any main branch of terrestrial phenomena according to the rules of analysis instituted by the immortal Bacon, will be repulsed by the idea of so many *elements* entering into the composition of the *electric fluid*, though hitherto almost excluded from the catalogue of *chemical substances* by a class of chemists, who confine their observations within their laboratories. When, with the view of ascending from some of the most common phenomena to general causes, we have followed this scrupulous analysis by a certain number of regular steps, we are yet, in almost every branch, stopped for want of intelligible *links*, though in series of phenomena manifestly connected together by some common cause; as are for instance many phenomena manifested in our chemical operations, with some which we daily observe in the atmosphere, that great laboratory of nature on our globe. The filling up of these chasms by gratuitous hypotheses is only protracting the attainment of real knowledge.

Nothing to stagger us in this compound nature of the electric fluid.

Let not therefore natural philosophers lose sight of an *expansible*

This expansi-

ble fluid produces various phenomena.

expansible fluid, constantly associated with all terrestrial bodies, and with the *air* that surrounds them; thus present in all our chemical processes, during which some of its *ingredients*, either *engaged* or *disengaged*, might account really for certain phenomena hitherto explained by mere words. For, according to meteorological observations which I shall relate in the following paper, it is by its *decompositions*, alternating with *compositions*, that the *electric fluid* operates in terrestrial phenomena. What were chemical theories before the *chemical* combinations of *fire* with other substances were discovered and attended to! However, as long as this *fluid* shall be considered under the vague idea expressed by the modern word *caloric*, it will not much forward the science of chemistry.

Transmission of electricity through interrupted conductors.

After these general remarks, I return to my subject, which will serve as an example of their application. When the transmission of the *electric fluid* through *interrupted* conductors takes place in a *liquid*, the new phenomena of *lucidity*, *heat*, and *odour*, are not perceived; but there cannot be any doubt, that the *chemical* effects produced in the *circuit*, and the *shock*, proceed from the same *decomposition* of particles, that takes place at *interrupted* conductors, which is visible only through the *air*; for no *chemical* effect is produced in the *water* of the glass tubes, when the *metallic wire* passes through it *uninterrupted*. With respect to the *shock*, this condition is not immediately perceived in the discharge of the *Leyden vial*, because it is sudden, attended with a strong commotion, and not repeated till the *vial* is again *charged*: but with the *pile*, which soon renews, spontaneously, the cause of the *shock*, it has been seen in Exp. 8, that this phenomenon is produced only at the *approaching* contact, and thus by an *interruption*; since all sensation ceased, when I *fixed* the silver spoons on both extremities.

The first question answered.

These preliminary deductions of facts were necessary for the solution of the first of the questions above stated, namely: "Of what *nature* is the modification produced in "the *electric fluid*, when it pervades a *pile* wherein the *cal*-"*cination* of some *metal* is going on?" a question intimately connected with this: "How does it happen, that, "with

" with such a minute quantity of *electric fluid* set in mo-" tion by the *pile*, the *shock* and *chemical* effects are pro-" duced, while they require a very great quantity of the " same *fluid*, when set in motion by any of the other known " electric apparatuses?" Being arrived at the general fact above stated, that these effects are never produced but by the *decomposition* of some particles of the *electric fluid*, occasioned by an excess of density, in darting from one point of a *conductor* to another, the answer to the connected questions is obvious: the *modification* undergone by the *electric fluid* in pervading this *pile* is such, that some of its particles are *decomposed* by a very small increase of *density*, when a *conductor* is *interrupted*. We have an analogy of the general case of more easy *decomposition* of *compounds* by previous modifications of the latter, in the processes of *smelting ores*, for obtaining *metals* or *reguli* from them; for an easy separation of the ingredient of the latter must be prepared, by *substraction* or *addition* of other ingredients, and often by both. And as we see, that the *calcination* by an *acid* is necessary to produce the *shock*, it is probable, that the modification of the *electric fluid* in this case is the addition of some *element*. For the shock some element added to the electric fluid.

I come now to the second of the above questions: " What " is the *cause* of a *motion* of the *electric fluid* in the *pile*? " either producing, or not producing, the *shock* and *che-* " *mical* effects in the *circuit*?" The first point to be considered with respect to this question concerns the *nature* of the modifications reciprocally produced by *zinc* and *copper* upon each other, when brought into contact. It is generally said, that, in this case, ZINC becomes *positive*, and COPPER *negative*. But these expressions, according to what has been stated above, cannot relate to the *expansive power* of the *electric fluid*; for, between two bodies in mutual contact, such a *power* must be in *equilibrium*; therefore these expressions must relate to *density*, such as I have defined it. Let us now attend to the experiments. 2d question.

These trials are usually made with disks of *zinc* and *copper* (or brass) having an insulating handle in their centre, like the plate of an *electrophorus*, and with the help of the *condenser*; thus, one of the disks being held on the hand, the other is first laid upon it, then brought, by its insulating handle. Contacts of zinc & copper.

handling into contact with the *condenser*. It is commonly supposed, that the latter must be *touched* while it lies on the other; but this, assimilating the process to that of the *electrophorus*, changes the nature of the phenomenon, and may lead into errour. I shall therefore first relate the experiments which I have made, with disks of the two metals about 4 inches diameter, from which the difference between these effects will be seen. But I must premise, that in these experiments, the results are very various, at different times, with respect to the *quantity* of effect; I shall therefore relate first such experiments as I set down in a certain part of one day; then mention the differences.

Exp. 30. *Exp.* 30. I held the *copper* plate on my hand, and laid upon it the *zinc* plate, which I *touched* with my finger in this situation, and then carried to the *condenser*: after 20 repetitions of these alternate contacts, the divergence, then *positive*, of the gold leaves of the latter, was about half an inch.

Exp. 31. *Exp.* 31. I made the same experiment without *touching* the *zinc* plate: the gold leaves struck the sides. I repeated the same process with only 10 alternate contacts, and the same quantity of effect was produced as with 20, when *touching* the *zinc* plate.

I must now mention, that at other times I have found no difference of effect from either *touching* or not *touching* the *zinc* disk while lying on the *copper* disk; and that at different times the quantity of effect in both operations was smaller; a point to which I shall return. but these first experiments prove directly, that when *zinc* is in communication with *copper*, the former takes from the latter some *electric fluid*, making other bodies, on its opposite side, share its *excess*. Thus, after a certain number of repetitions of the alternate contacts, when the upper plate of the condenser is removed, the *fluid* accumulated on the *receiving* plate is manifested by the electroscope: and instead of having increased the effect by *touching* the *zinc* on the *copper*, the finger has sometimes, even during that contact, taken off a part of the accumulated *fluid*. I come to the reverse experiment, made also at a certain part of one day.

Exp. 32. I took the *zinc* disk on my hand, and placing the *copper* disk on it, I *touched* the latter before carrying it

to the *condenser*, which it made *negative*. I repeated the experiment without *touching* the *copper* disk lying on the *zinc* disk, and the *negative* effect was of the same quantity. In general a greater number of alternate contacts is required to produce the same quantity of *negative* effect with *copper*, than of *positive* with *zinc*.

We see however in this experiment the same phenomenon as in the preceding, in this respect, that when *zinc* and *copper* are in mutual contact, the former takes some *electric fluid* from the latter, which, on its opposite side, it shares with other bodies: in the first experiment it shared this *fluid* with the *condenser*, while the *ground* restored it to the *copper* disk; in the last, *zinc*, communicating that acquisition to the *ground*, took more *fluid* from the *copper* disk, which made the *condenser* partake of its *loss*. This is the leading thread with respect to the *motion* of the *electric fluid* in the *pile*, and I shall follow it; but I must first speak more particularly of the anomalies observed in these experiments, which are important.

Anomalies in these experiments. These differences, sometimes very great, in the quantity of effect of the same operations, surprised me at first; and suspecting something amiss in the *condenser*, I examined it closely, without finding any defect: then at other times, without any change, I found the same effects. At last I remarked that, commonly, the greatest effects in the same day were in a part of the morning, and the smallest towards the evening; and that these effects differed also in intensity on different days. Now this is what I have said above of the spontaneous effects of the *pile*, which is composed of a succession of the same *binary* groups of *metals*; and thus the *condenser*, when applied to these experiments, is also a *meteorological* instrument. And there is a remarkable circumstance in this respect; that often at the same moment there is a great difference between the effects on the condenser of the opposite extremities of a small pile; but sometimes it is the *negative* side which prevails, and at other times it is the *positive*. This manifests, that the *ground*, with which the opposite side of the small pile communicates, in the former case possesses *less*, and in the latter *more*, of the *electric fluid* than the *ambient air*. But this will be one of the subjects of the following paper.

After

Propagation of the effects through the pile.

After having shown distinctly what are the *electric* effects produced by the conjunction of *zinc* and *copper*, and in what manner the condenser manifests these effects, I come to their propagation from each group along the *pile*, on one side *negative* and on the other *positive*. The following experiments will show the first steps, which will be easily extended through the whole.

Exp. 33.

Exp. 33. Upon a *zinc* disk, of the same size as the others, I fixed a piece of *Dutch-gilt paper*, the *copper* side next to the *zinc*, and the *paper* outside, held by a little paste all around. This association is to represent *one group* of the *pile*, considered for the present only on the *copper* side, with the *paper*, which, in the pile, separates it from the next *group* on this side, and more directly from the *zinc* plate of that *group*. Holding on my hand the *zinc* side of the former *group*, I repeated on its *paper*, with the insulated *zinc* disk, the alternate contacts with the condenser; and by 20 repetitions a sensible *negative* divergence was produced in the electroscope of the latter. The same effect takes place in the *pile* at the *copper* side of every *group*; it takes, through the *paper*, some *electric fluid* from the *zinc* of the *next group*; but as *zinc* must always possess more of this *fluid* than the *copper* with which it is connected, the *zinc* of that *next group* takes more from its own associated *copper*, which then, through the *paper*, takes more from the *zinc* of the following *group*; which effect goes on increasing, up to the end of the *pile* on this side. I shall only add to this experiment, that, as the property of a group composed of *zinc*, *copper*, and *paper* on the latter, is to have this side *negative*, whatever be the *metal* with which the above alternate contacts are performed, they produce the same *negative* effect on the *condenser*.

Condenser does not directly show, that the zinc yields electric fluid to the next group:

In order to complete directly the first step here in view, by taking one *group* as an example, it would be desirable, that, by placing a piece of paper on the *zinc* side of this group, in order to represent the separations of the *groups* in the *pile* in that direction, it should manifest directly by the *condenser*, that *zinc* yields some *electric fluid* to the next *group*; but I have not been able to obtain such a direct proof, on account of a circumstance which I shall now indicate, as peculiar to that quantity of *electric fluid* set in

motion

motion by the property of the *pile*. This *motion* is very *slow*, compared with the *motion* of an external quantity of *electric fluid*. The smallest degree of electrification of an insulated body, *positive* or *negative*, such as can only affect the gold leaf electroscope, communicated to one side of a *pile*, is instantly manifested at the other extremity; but it is by no means the same with respect to the *electric fluid* set in motion by the property of the *pile*: when the electroscopes diverge equally at its extremities, having touched one of them, which makes the gold leaves fall here, and rise more on the opposite side, it requires a time, often very long, before the same divergence is restored. This is the reason why I was obliged to fix 20 seconds for the duration of the contacts of my piles of 20 groups with the *condenser*, in order to be certain, that the *maximum* of effect was produced; it requires more or less *time*, according either to their nature, or to the extremity applied to the condenser; but I found, that the slowest was produced in 20 seconds, which made me fix that time.

because the motion of the fluid produced by the pile is very slow.

This will explain the case above mentioned, that there is no sensible effect on the *condenser* by any number of alternate contacts of an insulated body with the paper laid on the *zinc* side of one group; and even, when the *groups* are multiplied to increase the effect, it requires, to make it sensible, a prolongation of the contact on the little pile, in order to give *time* for the effect to be propagated; as will be seen by the following experiment.

Exp. 34. Made with a portion of my pile of 10 groups of 1·6 inch diameter, successively tried by each of their extremities, the opposite one being placed on the movable pillar of my condenser, and the alternate contacts being made with a small insulated disk of the same diameter. Exp. 34.

1. No number of alternate contacts, between either of the extremities of the little pile and the *condenser*, produced any sensible effect on the latter when rapidly made, or in the manner they are executed between the two naked disks, zinc and *copper*.

2. On repeating the experiment, with the *zinc* side on the *pillar*, and lengthening the *time* of the contact on the *copper* side up to 4 *seconds*, I found, that by 20 such contacts the

electroscope

electroscope was affected *negatively* in a certain measurable quantity.

2. Inverting the little pile, I had no sensible effect by these contacts of 4 *seconds* on the *zinc* side, and it was necessary to lengthen the time to 8 *seconds*; but in order to produce on the electroscope a *positive* effect equal to the *negative* of the preceding trial, I was obliged to make 40 contacts, on account of the dissipation of the effects on the condenser in each interval of time. This experiment shows however sufficiently, that the *zinc* side of each *group* yields, through the *paper*, to the next *group*, some of the *electric fluid* that it takes from the *copper* with which it is associated.

Elementary principles of the motion of the fluid in the pile.

We have from these experiments all the elementary principles necessary for the *motion* of the *electric fluid* in the *pile*, and they are the following.—1. In each *binary group*, the *zinc* plate takes some electric fluid from its associate the *copper*; the latter, in my new *pile*, being the *coppered side* of the *Dutch-gilt paper*. 2. In each group also, *zinc* communicates, through the *paper*, some of its excess of *fluid* to the *copper* of the next *group* on its side. 3. In each *group* again the *copper* takes, through the *paper*, from the *zinc* of the next *group* on its side, some of the *fluid* that it has lost to its associated *zinc*. The same effects taking place in every *group*, with the next on both *sides*, along the whole *pile*, these effects are successively added to those that the respective *next groups* have already undergone, according to their *place*; and thus the *negative* state goes on increasing from one end to the other of the *pile*, toward what is called the *copper* extremity; and the *positive* state is increasing toward the *zinc* extremity.

These effects may be represented by *numbers*; though, from the great variations in the quantity at different times, and the imperfection of the electroscopes, these *numbers* remain undetermined: I shall express them in a *pile* of 11 *groups*, indicating by A the *zinc side*, and by B the *copper side*. The two following *series* represent the progress of *negative* and *positive* effects above mentioned, which, continuing in each successive *group* from A to B, constitute, according to circumstances, the three different states of the *pile* afterward figured.

A	A
0	+ 10
− 1	+ 9
− 2	+ 8
− 3	+ 7
− 4	+ 6
− 5	+ 5
− 6	+ 4
− 7	+ 3
− 8	+ 2
− 9	+ 1
− 10	0
B	B

Different states of the pile.

In the insulated pile, when the divergence of the electroscopes is equal on both sides, positive at A, and negative at B, the state of each successive group is the sum of the correspondent numbers of the above series, as expressed in the 1st of the following Tables.—When B is placed in communication with the ground, the losses of all the copper plates being repaired by the latter, all the acquisitions of the zinc plates subsist without diminution, which requires the quantity 10, to be added to each number of Table I, as expressed in Table II.—When A communicates with the ground, all the acquisitions of the zinc plates being carried into the latter, the losses of the copper plates remain uncompensated, and the same quantity, 10, is to be substracted from all the numbers of Table I, as expressed in Table III.

Table I.	Table II.	Table III.
Insulated pile.	*B in com. with the gr.*	*A in com. with the ground.*
A	A	A
+ 10	+ 20	0
+ 8	+ 18	− 2
+ 6	+ 16	− 4
+ 4	+ 14	− 6
+ 2	+ 12	− 8
0	+ 10	− 10
− 2	+ 8	− 12
− 4	+ 6	− 14
− 6	+ 4	− 16
− 8	+ 2	− 18
− 10	0	− 20
B	B	B

Now

Now this synthesis of the above fundamental experiments is the real fact; as will be seen from direct experiments in the following paper, of which I shall give here only the general results.

proved by experiment.

For these experiments I use a horizontal *pile*, which I have called *column*, with a gold leaf electroscope at each extremity; and I have also a detached electroscope, which may be applied and observed at every point of the *column*. The following are the observed phenomena.—1. When the state of the ambient air is such, that in the *insulated column*, the divergence is equal at both extremities, the *middle* point in its length is *zero*, as represented in Table I, in which, as well as in the two others, the *terms* are to be considered only as equidistant *points*, whatever be the number of the *groups*.—2. When B communicates with the *ground*, the first plate only at this extremity is *zero*, and the *positive* state is gradually increasing towards A: the *middle point* is *plus*, of the same quantity (sensibly) as it was at A in Table I, and the divergence *plus* is doubled at A; a state represented in Table II.—3. When A communicates with the *ground*, all the effects are reversed: the first plate only at A is *zero*, and the *negative* state is gradually increasing towards B: the *middle point* is now *minus*, of the same quantity (sensibly) as it was at B in the case of Table I, and the quantity *minus* is doubled at B; a state represented by Table III. I do not know any theory on *invisible causes*, which more exactly follows the *visible effects*.

Physical cause of these phenomena.

What remains to be considered is the *physical cause* of these phenomena, all originating in this circumstance, that when *zinc* and *copper* are in mutual contact, *zinc* possesses more *electric fluid*, and *copper* less, than in another situation. In my first paper delivered to the Royal Society, I explained this effect by analogy with the phenomenon of the different *capacities* of bodies for the *fluid* cause of *heat*; but having here entered into an explanation of the nature of the *electric fluid*, I shall derive analogies from the subject itself.

Most electric phenomena depend on the density of the fluid.

Most of the electric phenomena manifested in our experiments depend on the distinction, which I have established, between the *density* of the *electric fluid*, consisting in the proportional quantity of *electric matter* and its *expansive* *power*,

power, depending on the quantity of *vector*. This distinction is particulary manifested by the *changes*, that happen along an insulated *conductor* of some length, when an electrified body is placed at some distance from one of its extremities. It is known in general, that the extremity of such a *conductor* next to the *electrified body* acquires an *electric state* contrary to that of the *body*, while its opposite extremity has the same electric state as that body: for instance, suppose the electrified body to be *positive*; it is commonly said, that the extremity of the *conductor* next to this *body* becomes *negative*, and the opposite extremity *positive*; but these are vague expressions, and as they give no real idea of the effects, they have occasioned the variety of systems, all unsatisfactory and therefore changing, hitherto made on these phenomena.

Distinction between its expansive power and density necessary.

The cause of obscurity on this object is the want of that distinction above mentioned, between the *expansive power*, and the *density* of the *electric fluids*, belonging to its nature, as *expansible*. Every fluid of this class, when confined within a certain space, has necessarily the same degree of *expansive power* in every part of this space, since this is attached to the very idea of *expansibility*; but it is not the same with respect to *density*. For instance: a mass of *air*, confined in a certain space, has certainly, in all its parts, the same *degree* of *expansive power*, whatever change may happen in its partial *density*. If then a *hot body* be placed near one side of this mass of *air*, its *density* will *diminish* in this part, and *increase* in the more remote: or if a piece of *ice* be brought on one side of this mass of *air*, its *density* will *increase* near the *ice*, and *diminish* in the remote parts; but the *degree* of *expansive power* of this confined *air* will always be equal in all its parts at the same time, *increasing* or *diminishing* in the whole.

Illustrated by the action of heat.

Instanced in electricity.

The case is exactly the same with respect to the *electric fluid* on an insulated *conductor*: an insulated *positive* body being placed near one part of the latter, this part receives some of the *vector* which forms around the *positive body* a kind of atmosphere, as the *hot body* has around it an atmosphere of *igneous fluid*; and the former produces an increase of the *expansive power* of the *electric fluid* on this part of the *conductor*, as the *hot body* produces it in the *air* next

next to it. A *negative body* produces also an effect analogous to a piece of *ice*; for as this absorbs a part of the free *fire* in the parts of the mass of *air* next to it; so the *negative body* absorbs a part of the *vector* from the *electric fluid* on the nearest part of the *conductor*. Now, all these changes in the *degree* of *expansive power* in both *fluids* are attended with inverse changes in their *density*.

This proved by experiments

Nobody will doubt of the above statement, who has repeated the experiments described in my works; and I may say, that every one of the great number of persons, in whose presence I have made these experiments, with the set of small electric instruments described in my *Traité élémentaire sur le Fluide Electro-galvanique*, has been convinced of this important distinction between the *density* and *expansive power* of the *electric fluid*. It is not therefore for want of progress in the science itself, that such variety of systems subsists concerning the *electric phenomena*, it is for want of attention in most experimental philosophers, by whom, though writing on *electricity*, my published experiments are never mentioned, not so much as to criticise them, or their *conclusions*. Among the variety of these experiments, I have here chosen those concerning the modifications which take place on a long insulated *conductor*; and I come now to the particulars, which will prove all that I have stated above.

Three *electroscopes* are used in that series of experiments, one of which, a *movable* one, consists of silver *laminæ*, suspended on small axes, in order to prevent their motion out of the line of their divergence. Of the two other *electroscopes*, consisting of small pith balls suspended by silver wires, one is permanently connected with the extremity of the *conductor* farthest from the *electrified body*; the other, held up at the top of a high insulating pillar, is kept, by means of a thin wire, in constant communication with the *movable electroscope*: the latter, though principally destined to move along the *conductor*, may be removed, from every point, to a distance, in order to try the *nature* of the *divergence* that it had at this point, which it does not lose on removal. Lastly an insulated brass *disk* is the *electrified body*. The following are the phenomena observed, in which must be recollected what I undertake to prove, namely, that

Points to be proved.

that the *divergences* of the *electroscopes* are produced by *plus* or *minus* of *electric matter* only, comparatively with the *standard*, which is the *actual* proportional quantity of *electric matter* possessed by the *ambient air;* an object connected with the proof of the fundamental proposition, that there is an absolute distinction between the *density*, and the *expansive power* of the *electric fluid*.

1. At the beginning of the experiment, the *movable electroscope* is placed near that extremity of the *conductor*, to which the disk, after having received a spark from a Leyden vial, is to be approximated. At the approach of this *positive body* within a small distance, the silver *laminæ* of this *electroscope* diverge as *negative*; some of their *electric matter* having receded to the remote parts of the *system* (by this word I express the *conductor* and its associated *electroscopes*); and thus, though the *expansive power* of the *electric fluid* has equally increased upon its whole extent, the two remote *electroscopes*, and in particular that which is connected with the *silver laminæ*, diverge *positively* by a certain quantity. Phenomena.

2. When the *silver laminæ* are made to *recede* from the *positive* body along the *conductor*, their *negative* divergence gradually diminishes; it ceases at a certain distance, and farther than this begins a *positive* divergence, which increases to a certain *maximum;* nevertheless the remote electroscopes, and in particular that which is in immediate connection with the *laminæ*, remain *positive* to the same degree as at first. The effect, therefore, of withdrawing the *laminæ* has been to remove them out of the atmosphere of *vector* of the *positive body*, which, by increasing the *expansive power* of their *electric fluid*, had made a part of their *electric matter* to abandon them: and it is the *electric matter* retired from the anterior part of the *system*, which occasions the *positive* divergence in the remote *electroscopes*.

3. While the *movable electroscope* is thus removed from the atmosphere of the *positive body*, if any part of the *system* be touched with a small wire held in the hand, the *expansive power* of its *electric fluid* is thus placed in *equilibrium* with that of the *ground*; and the *divergence* ceases in all the *electroscopes*. Which shows, that, in the parts of the *system* over which the atmosphere of the *positive body* does

does not extend, the *equilibrium* of proportional quantity of *electric matter* has been also produced. If then the *silver laminæ* be moved toward the *positive body*, when they arrive within that atmosphere they begin to diverge as *negative*; and this divergence continuing to increase, it is greater when this electroscope arrives at the end of the *conductor*, than it had been in the same place at the beginning of the experiment. However, the remote *electroscope* in communication with this remains without divergence; because the small quantity of *electric matter* newly withdrawn from the *silver laminæ* is insensible upon the whole *system*.

4. Now will come a proof (among many others which may be found in my works), that the *divergence* in the electroscopes depends *only* on the proportional quantity of *electric matter*, or *density* of the *fluid*. If, in this state of the *system*, the *silver laminæ* be touched with a wire held in the hand; though this contact places them in communication with the *ground*, their *divergence* continues the same: because their *electric fluid*, by the increase of *vector* proceeding from the *positive body*, being *in equilibrium* of *expansive power* with that of the *ground*, no *electric matter* can ascend to them from the latter, and they remain *deprived* of it to the same degree.

5. Lastly. If the *positive body* be removed or discharged, the three electroscopes diverge as *negative*. By the contact of the *system*, during the influence of the *positive body*, which had increased the *expansive power* of the *electric fluid* over the whole, the quantity of *electric matter*, which had retired from its anterior parts, had passed into the *ground*; and now, when the influence of the *positive body* has ceased, this *deficiency* of *electric matter* becomes common to the whole system.

Fundamental proposition.

Having now, by this series of experiments, demonstrated the fundamental proposition, that an *equilibrium* of *expansive power* of the *electric fluid* may subsist, between two insulated bodies in mutual contact, with a difference in its *density*, or proportional *quantity of electric matter*, I come to my system of the *electric states* of *zinc* and *copper*, when, being insulated, they are in mutual contact, which system is founded on that proposition.

of the It is evident, that the *electric fluid* must be in *equilibrium*

brium of *expansive power* on this group composed of *zinc* and *copper*; therefore, the *difference* observed in their *electric states* must proceed from a difference in the *density* of the *fluid*. Now, the only hypothesis added in my system to this immediate conclusion from fact is this: that, during their connexion, *copper* has the property of acquiring more *vector* than *zinc*, from that diffused in the *ambient air;* by which proportional increase of *expansive power*, the *electric fluid* on *copper* is in equilibrium with that on *zinc*, though with less *density*, or proportional *quantity* of *electric matter*. I have shown also in the above mentioned works, by deduction from experiments, that, when this *influence* of bodies on each other, by *plus* or *minus* of *vector*, has ceased by sufficient *distance*, each of them possesses instantly, by the effect of the *ambient medium*, a quantity of *vector*, proportional to its quantity of *electric matter*; and thus it is, that the modifications produced by *zinc* and *copper* on each other, while associated, and their effects on bodies brought into contact with them on the outside of their *groups* during their association, are converted into modifications of the *quantity* of the *electric fluid* itself. electric states of insulated zinc and copper in contact.

After having treated here the theoretical part of the subject more fully than I had done (for brevity's sake) in my first paper given to the Royal Society; in order to be better understood on this subject, very important in natural philosophy; I return to the experiments concerning the *analysis* of the *galvanic pile*, to bring them here to the same point as they were at in that paper.

Having found, by the experiment related at the end of the first *part* of this analysis, that, by increasing the *size* of the *plates*, the *divergence* did not increase in the electroscopes, I considered the manner in which a great number of small plates might be used. I thought then of having a hole in the centre of small plates, in order to thread them with silk in form of *chaplets*, alternating the plates with equal pieces of *Dutch-gilt-paper*. Not having yet any but *tinned iron plates* for these trials, I formed two such *chaplets*, each composed of 140 *groups*, of 0·5 inch diameter, and in order to guard them against dust, I enclosed them in glass tubes: but I found, that, when the *chaplets* Contrivance for employing a great number of small plates.

lay

lay along the glass, the effect was diminished, and in order to prevent this defect, I took larger tubes, with metallic caps, through which, by screws, I kept the *chaplet* fixed in the axis of the tube; these screws, being on the outside in the form of hooks, served to link the *chaplets* together. Each of these small instruments acted immediately on the gold leaf electroscope, and, by hooking them together, the effect was doubled.

I founded at that time, on this experiment, the plan of increasing the power of the new instrument so as to produce the divergence of small metallic balls, especially by using *zinc* plates, which I knew then could be procured. For this purpose, I thought of lessening the expense occasioned by the metallic caps for the glass tubes, by making much longer *chaplets*, and of suspending these in the form of garlands, to the ceiling of the room, by silk strings, bringing only wires from their opposite extremities to a proper place, where the apparatus of the small metallic balls should stand.

Electric columns and aerial electroscope.

Such is the point at which I had arrived the 30th of May, 1808; and this object was much forwarded in my paper under the title of *The Electric Column and Aërial Electroscope*, delivered to the Royal Society the 7th of March, 1809; but the Committee of Papers not having ordered it to be published in the Phil. Trans., it will now appear in a more advanced state. The consequence of its not appearing at the time it should have done is, that my *electric column* has lost the merit of novelty; for by the communications of the minutes of the meetings of the Society and the Committee, it had attracted attention; and the sight of it, which I did not refuse, has made it sufficiently known to be already imitated. However, while its connexion with the analysis of the Galvanic pile is not considered, its principal object is lost: and moreover, till the *sticking* of the small *metallic pendula*, when *striking*, was prevented, which I have obtained but lately, it could not be ranked among *meteorological* instruments. These particulars will be seen in my following paper.

Windsor, 22d of June, 1810.

II.

II.

On the Botryolite, or Grapestone: by Count DUNIN BORKOWSKI *.

THIS stone is only found in mass, and its external figure is uniform. From this figure its name is derived. Botryolite.

Its colours are pale rose, pearl gray, yellowish white, ashen gray, and Isabella yellow. All these colours alternate in very thin laminæ; the deepest colour in general forming the outermost coat. Its characters.

Both externally and internally it is without lustre.

Its fracture is with slender, divergent fibres. It becomes scaly.

Sometimes it is translucid throughout, sometimes only at the edges.

It is semihard, scratching glass only in a slight degree; and brittle.

Its specific gravity is 3·000.

Before the blowpipe it is fusible with ebullition.

It contains boracic acid. Contains boracic acid.

It is found accompanied with quartz, black schorl, carbonate of lime, martial pyrites, and magnetic iron, in the mine of Kienlie, near Arendahl, in Norway. Where found.

III.

An Analysis of several varieties of British and Foreign Salt (Muriate of Soda), with a view to explain their Fitness for different economical purposes. By WILLIAM HENRY, *M. D. F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary at Manchester.*

(Concluded from p. 206.)

SECT. III. *Account of the Methods of Analyzing the several Varieties of Muriate of Soda.*

THE method of analysis, which I adopted, in examining the several varieties of muriate of soda, was as follows.

* Journal de Physique, vol. LXIX, p. 159.

Method of analysing the muriate of soda,

When the salt was in a state of solution, a measured quantity was evaporated to dryness in a sand heat, which was carefully regulated, to avoid the decomposition of the muriate of magnesia, if any of that salt were present in the solution*.

Each specimen of salt was reduced to a fine powder, and was dried, in the temperature of 180° of Fahrenheit, during the space of two hours. This was done in order that the different experiments might be made on precisely equal quantities of salt.

I. *To separate the earthy Muriates.*

Separation & calculation of the earthy muriates.

(A) On 1000 grains of the dried and pulverized salt, (except in the case of the foreign salts, when only 500 grains were used,) four ounce measures of alcohol were poured, of a specific gravity varying from 815 to 820, and at nearly a boiling temperature. To insure the access of the fluid to every part of the salt, they were ground together for some time in a mortar, and then transferred into a glass matrass, where they were digested for some hours, and frequently agitated. The alcohol was next separated by filtration, and the undissolved part was washed, as it lay on the filtre, with 4 ounce measures of fresh alcohol.

(B.) The united washings were evaporated to dryness†, and to the dry mass a small portion of fresh alcohol was added, to separate the earthy muriates from a little common salt, which had been dissolved along with them. This second solution might, however, still contain a minute portion of muriate of soda. It was therefore again evaporated, redissolved in hot water, and mixed with a solution of carbonate of soda. By boiling for some minutes, the whole of the earths were precipitated, and after being well washed, were redissolved in muriatic acid. This solution, being

* Muriate of magnesia, according to Dr. Marcet, begins to part with its acid at a temperature a few degrees above that of boiling water. This fact explains the observation of Mr. Kirwan, that too great a heat, employed in the desiccation of muriate of magnesia, decreases considerably its solubility in alcohol. (Kirwan on Mineral Waters, p. 275.)

† In this and all similar cases, the heat was very cautiously regulated towards the close of the process.

evaporated

evaporated to dryness, gave the weight of the earthy muriates, which had been extracted by alcohol*.

(B. a.) The dry mass thus obtained might consist either of muriate of magnesia, or muriate of lime, or of both. An aliquot part, therefore, was dissolved, separately, for the purpose of assaying it by the usual tests. Sometimes, as in the case of the earthy muriates procured from sea salt, muriate of magnesia alone was indicated, and any farther process was rendered unnecessary. Muriate of lime was in no instance found uncombined; but in the majority of cases (as in the earthy muriates obtained from Cheshire salt) was mixed with muriate of magnesia.

(B. b.) To the solution of the two earthy muriates was added fully saturated carbonate of ammonia, which has the property of throwing down lime in combination with carbonic acid, but has no effect on the muriate of magnesia at ordinary temperatures. The solution of the latter salt, along with that of the excess of carbonate of ammonia, was therefore separated by filtration; and to the filtered liquor a solution of phosphate of soda was added, according to the formula of Dr. Wollaston†.

(B. c.) By direct experiments I had learned, that 100 grains of muriate of magnesia, when thus decomposed by carbonate of ammonia, conjoined with phosphate of soda, give 151 grains of insoluble ammoniaco-magnesian phosphate dried at about 90° of Fahrenheit. Hence it was easy, from the weight of the precipitate, to calculate how much of the former salt was contained in the mixture of muriate

* By the analysis of artificial mixtures of pure muriate of soda with the earthy muriates in known quantities, I afterward found, that the full amount of the earthy muriates was not ascertained in this way of proceeding. The deficiency of the latter salts was about one sixth; but as the errour must necessarily have been the same in all, it does not affect the comparison of different varieties of salt, as to their proportion of this ingredient. If the numbers in the 5th column of the table (indicating the total earthy muriates) be increased in the proportion of six to five, we shall then obtain the true quantities in each variety of salt. Full amount of the earthy muriates given too small.

† See Dr. Marcet's analysis of the Brighton Chalybeate, published in the last edition of Saunders on Mineral Waters.

of lime and muriate of magnesia. Thus, if 20 grains of a mixture of the two muriates yielded 15·1 of ammoniaco-magnesian phosphate, it is obvious, that the mixture must have consisted of equal weights of muriate of lime and muriate of magnesia.

(B. d.) The estimation of the proportion of muriate of lime, in a mixture of this salt with muriate of magnesia, was sometimes performed in a different way. To a cold solution of a known weight of the two salts, superoxalate of potash was added; and the precipitate was collected, washed, and dried at about 160° Fahrenheit. Of this precipitate I had previously found, that 116 grains are formed by the decomposition of 100 grains of dry muriate of lime. From the quantity of oxalate of lime it was easy, therefore, to infer that of the muriate, from the decomposition of which it resulted; and this, subtracted from the weight of the two salts, gave the weight of the muriate of magnesia.

II. *To separate and estimate the earthy Sulphates.*

Separation and calculation of the earthy sulphates.

(C.) The portion of salt, which had resisted the action of alcohol, was dissolved by long boiling in sixteen ounce measures of distilled water, and the solution was filtered. On the filtre a small quantity of undissolved matter generally remained, which was washed with hot water, till it ceased to have any action. The weight of the insoluble portion was then ascertained.

(C. a.) By this operation were dissolved, not only the muriate of soda, but all the other salts, insoluble in alcohol, which might be mingled with it. To the solution carbonate of soda was added; and the liquid, which in most cases gave, on this addition, an abundant precipitate, was boiled briskly for several minutes, in order that none of the earthy carbonates, which were separated, might remain dissolved by an excess of carbonic acid.

(C. b.) The precipitated earths were allowed to subside, and were well edulcorated with boiling water, the washings being added to the liquor first decanted from the precipitate. To these united liquids (after the addition of more muriatic acid than was required for saturation) muriate of barytes was added, till it ceased to occasion any further precipitate.

The

The sulphate of barytes was then washed sufficiently; dried, ignited, and its amount ascertained.

Separation and calculation of the earthy sulphates.

To the earthy carbonates an excess of sulphuric acid was added in a platina dish, and the mixture was triturated, till all effervescence ceased. It was then evaporated to dryness, calcined in a low red heat, and the weight of the earthy sulphates was ascertained.

(D. a.) The dry sulphates were washed with a small quantity of lukewarm water. In several instances, the loss of weight, thus sustained, was extremely trifling, nothing being dissolved but a very minute portion of sulphate of lime, of which earthy salt, solely, the residue was presumed to be composed.

(D. b.) But in other cases, a considerable loss of weight ensued; and in these, to the watery solution was added a mixture of equal parts of saturated solutions of carbonate of ammonia, and phosphate of soda. A precipitate more or less copious was produced, which was collected, dried at 90° Fahrenheit, and weighed.

(D. c.) By direct experiments I had determined, that 90 grains of this precipitate result from the decomposition of 100 grains of sulphate of magnesia, of such a degree of dryness, as to lose 44 grains out of 100, by exposure to a low red heat. Hence 100 grains of ammoniaco-magnesian phosphate indicate 111 grains of crystallized, or 62·2 of desiccated, sulphate of magnesia*. From the weight of the ammoniaco-magnesian phosphate, it was easy, therefore, to infer the proportion of sulphate of magnesia in any mixture of the two earthy sulphates.

(D. d.) It was possible, however, that, in addition to the sulphates of lime and of magnesia, the quantity of which had been determined by the foregoing process, the specimen of salt under examination might contain also an alkaline sulphate. To decide this point, it was necessary to

Water of crystallization in sulphate of magnesia.

* The assumption, that crystallized sulphate of magnesia contains only 44 per cent of water, though it was correctly true with the specimen on which I operated, is below the average; which, I find from several experiments, is about one half the weight of the salt. Mr. Kirwan states the water of crystallization to be 53·6 in 100 grains; but this, I believe, a little exceeds the truth.

compare

Separation and calculation of the earthy sulphates.

compare the amount of the acid, deducible from the weight of the sulphate of barytes (C. b.), with that which ought to exist in the sulphate of lime and sulphate of magnesia, actually found by the experiment. But, to make this comparison, some collateral experiments were previously necessary.

(D. e.) By these experiments I found, that sulphate of lime prepared by double decomposition, then calcined in a low red heat, and afterward dissolved in a large quantity of boiling distilled water, yields, when precipitated by a barytic salt, in the proportion of 175·9 grains of sulphate of barytes from 100 of the calcareous sulphate*. The same quantity of ignited sulphate of lime (= 128 grains dried at 160° Fahrenheit,) precipitated by superoxalate of potash, gives 102·5 of oxalate of lime; or, precipitated by subcarbonate of potash at a boiling heat, 74·3 grains of carbonate of lime†. One hundred grains of crystallized sulphate of magnesia (= 56 desiccated) afford, when precipitated by muriate of barytes, 111 or 112 of the barytic sulphate.

(E.) By a comparison of the above proportions with those obtained in the analyses of any specimen of common salt, we may learn, whether it contain other sulphates beside those with earthy bases. For example, if the precipitate (D.) consist of carbonate of lime only, and bear to the sulphate of barytes (C. b.) the proportion of 74 to 175, or very nearly so, we may infer, that no other sulphate is present, but that of lime. The same conclusion will follow, if, after having decomposed one half of the watery solution (C.) by muriate of barytes, and another half by potash, we find

* This result corresponds, within a fraction of a grain, with one obtained in a somewhat different way by Dr. Marcet; and very nearly with an experiment of my friend Mr. James Thomson, who found the barytic sulphate, precipitated from 100 grains of sulphate of lime by nitrate of barytes, to weigh 173 grains.

† On reversing this experiment, I found that 100 grains of carbonate of lime, saturated with sulphuric acid, and calcined in a low red heat, afford 135 of sulphate of lime. A similar experiment of Mr. Thomson gave 134·6 grains. Dr. Marcet, also, informs me, that from 98·55 grains of pure marble he obtained 125·95 grains of sulphate of lime, proportions which exactly coincide with those of Mr. Thomson.

that

that the sulphate of barytes bears to the oxalate of lime the proportion of 175·9 to 102·5. Now these proportions were, as nearly as could be expected, obtained in the analysis of Northwich salt; whence we may conclude, that the only sulphate, which it contains, is gypsum, or the sulphate of lime.

Separation and calculation of the earthy sulphates.

It must be remembered, however, that the calcareous sulphate, contained in any variety of common salt, cannot be in a state of complete desiccation, but would lose 22 parts out of 100, by exposure to a red heat*. It becomes necessary, therefore, either to increase, in the proportion of 5 to 4, our estimate of the sulphate of lime obtained by the foregoing rule, or, more simply, to assume, that 100 grains of sulphate of barytes indicate 73 grains of sulphate of lime, dried at 160° Fahrenheit, = 57 ignited.

(F.) When sulphate of lime and sulphate of magnesia were both ascertained, and other sulphates also might possibly be present, as in the varieties of salt from sea water, the calculation became a little more complicated. In this case, after determining the quantity of both sulphates, (by the processes D. &c.) I estimated how much sulphate of barytes they ought respectively to afford; and then compared the estimated quantity with that which was actually obtained. The earthy carbonates, for example, precipitated from 1000 grains of Lymington salt, which had previously been digested with alcohol, were converted into 31 grains of calcined sulphates, consisting of 19 grains of dry sulphate of magnesia, and 12 grains of dry sulphate of lime. Now from the magnesian sulphate 38 grains of sulphate of barytes should result; and from the sulphate of lime 21 grains; the sum of which is 59. But the quantity actually obtained was 59·8. There is only, therefore, an excess of 0·8 grain of the actual above the estimated quantity, a difference much too trivial to be admitted an indication of any sulphate with an alkaline base; and arising, probably, from unavoidable errours in the experiment,

* This I find to be the loss sustained by 100 grains of artificial selenite, dried at 160°, and then ignited. The same quantity of crystallized native selenite, I learn from Dr. Marcet, loses 20·7 grains, by being calcined in a strong red heat.

(F. a.)

(F. a.) If in any mixture of salts, free from the earthy muriates, we are certain that no other sulphates exist beside those of lime and magnesia, their estimation becomes extremely simple. Decompose two equal quantities of the salt in question, the one by muriate of barytes, the other by oxalate of potash. From the weight of the latter precipitate we may calculate the quantity of sulphate of lime. Suppose, for example, the oxalate of lime (as was actually the case with the precipitate from 1000 grains of Lymington salt,) to weigh 12 grains: these denote 15 of sulphate of lime, dried at 160° Fahrenheit; which quantity, if decomposed, would give 20½ of sulphate of barytes. The latter number (20½), subtracted from the weight of sulphate of barytes actually obtained (say 60), gives 39½ grains for the sulphate of barytes resulting from the decomposition of the sulphate of magnesia. The quantity of the latter salt, it will be found, therefore, by applying the rule already given (D. e.), must be 35 grains.

(F. b.) The same object may be accomplished by decomposing two equal quantities the one by oxalate of potash, the other by the compound solution (D. c.) From the weights of the precipitates it is easy to calculate, from how much of the calcareous and magnesian sulphates they have resulted.

Separation of alkaline sulphates.

(G.) When the salt left by alcohol was known to contain muriate of soda, and sulphate of magnesia, but no sulphate of lime the presence of alkaline sulphates was investigated in the following manner. The salt was dissolved in water, and the solution was divided into two equal portions. To the one muriate of barytes was added, and to the other, the compound precipitant of carbonate of ammonia, and phosphate of soda. If the sulphate of barytes, thus produced, bore to the ammoniaco-magnesian phosphate the proportion of 112 to 90, it was concluded, that no other sulphate had been decomposed, but that with base of magnesia.

(H.) At one time, I expected to have ascertained the quantity of sulphate of soda, in an artificial mixture of that salt with sulphate of magnesia and muriate of soda, by the following formula. To a solution of the three salts, heated to a boiling temperature, I added subcarbonate of ammonia, which

which decomposes the sulphate of magnesia only. I had then a solution containing muriate and sulphate of soda, with sulphate of ammonia, and some carbonate of ammonia. This solution was evaporated to dryness, and the mass was sufficiently heated to expel the ammoniacal salts. I found, however, that at this temperature the sulphate of ammonia acted upon the muriate of soda, and produced an additional, and not inconsiderable quantity of sulphate of soda.

Impurities deducted gave the weight of muriate of soda.

Having determined, by the foregoing processes, the quantity and kind of the earthy muriates, the amount of the insoluble matter, and the proportion of sulphates, the weights of all these different impurities were added together; and, the sum being deducted from the weight of the salt submitted to experiment, the remainder was assumed as the amount of the pure muriate of soda in the specimen under examination*.

Additional circumstances.

Though I purposely refrain from giving the details of the several analyses, which were made according to the foregoing plan, from the conviction that they would be both tedious and unnecessary, yet there are a few circumstances, which it may be proper to mention more fully than can be done in the form of a table.

Brine from Northwich.

1. The *brine* which I examined was from Northwich, and was sent me in the state in which it was taken from the spring†. At the temperature of 56° Fahrenheit, it had the specific gravity of 1205. It was perfectly limpid, but lost a

* I have deemed it unnecessary to state, in the table, the quantities of the acid and base in several varieties of muriate of soda. They may readily be estimated from the proportion, deduced by Dr. Marcet, of 46 acid, and 54 soda, in 100 of the pure muriate. In this determination he assumes, that 100 parts of luna cornea, after being melted and heated to redness, consist of 19·05 parts of acid to 80·95 oxide of silver. This statement agrees very nearly with the recent one of Gay-Lussac, who makes 100 parts of silver to combine with 7·6 oxigen, and this oxide to neutralize 25·71 parts of real muriatic acid.

† I have lately been informed, that this brine had been pumped out of a rock-salt mine, into which, from the impossibility of obtaining the salt in a solid form, it was allowed to flow. Hence it was fully saturated with muriate of soda.

little

little of its transparency when raised to a boiling heat, in consequence of the deposition of a very minute quantity of carbonate of lime, and oxide of iron. It was immediately precipitated by muriate of barytes, oxalate of ammonia, and alkaline solutions, both mild and caustic. Eight ounce measures, evaporated to dryness in a sand heat, gave 1230 grains of salt, which, for the sake of distinction, I term *entire* salt. It proved, on analysis, to contain in one thousand parts*,

Composition of its salt.

Carbonate of lime and oxide of iron	2
Muriate of lime, and muriate of magnesia, in nearly equal proportions	5
Sulphate of lime	19
Muriate of soda	974
	1000

Mother liquor.

2. The *mother liquor*, or brine that remains after separating all the common salt, which it is thought worth while to extract, had the specific gravity of 1208. The dry salt contained

Muriate of magnesia	35
lime	32
Sulphate of lime	6
Muriate of soda	927
	1000

Clearings of the brine.

3. The *clearings* of the brine, which are raked out of the pan when the salt first begins to granulate, contained in 1000 parts,

Muriate of soda	800
Carbonate of lime	41
Sulphate of lime	159
	1000

Pan scale.

4. Of the substance called by the workmen *pan-scale* two specimens were analysed, the one containing a large propor-

* The specific gravity and proportion of earthy sulphates in Cheshire brine appears to differ considerably in the brine of different springs. See Holland's Cheshire Report, p. 45, &c.

tion

tion of muriate of soda, the other very little. The first variety consisted of

Muriate of soda	950
Carbonate of lime	10
Sulphate of lime	40
	1000

The second variety was composed of

Muriate of soda	100
Carbonate of lime	110
Sulphate of lime........	790
	1000

Circumstances, however, are constantly occurring, to vary the proportion of ingredients, both in the clearings and in the pan-scale. If, for example, the brine be short of the point of saturation with common salt, it acts, when admitted into the pan, upon the muriate of soda which the pan-scale contains, and we obtain the second variety. But if the brine be fully charged with salt, it affects no solution of the muriate of soda carried down along with the gypsum; and then the first species of pan-scale results.

5. The *salt oil*, or mother liquor from seawater, a specimen of which I received from Dr. Thomson, had the specific gravity of 1277. It was abundantly precipitated by muriate of barytes: by pure ammonia, but not by the carbonate; and was not changed by oxalate of potash, either immediately or after an interval of some hours. One thousand parts of the dry salt consisted of

Salt oil, or mother liquor of seawater.

Muriate of magnesia	874
Sulphate of magnesia....	70
Muriate of soda	56
	1000

6. The *salt brine*, or liquor which drains from the Scotch salt, had the specific gravity of only 1188. It was affected by the same tests as the salt oil, but less remarkably. The dry residue contained

Salt brine, or liquor drained from Scotch salt.

Muriate

Muriate of magnesia	205
Sulphate of magnesia....	135
Muriate of soda	660
	1000

Mother liquor, or bittern, from Lymington.

7. The *mother liquor*, or *bittern, from Lymington*, presented, on analysis, an unaccountable variation from the similar fluid sent from Scotland, and gave a much larger proportion of sulphate of magnesia. A considerable quantity of this salt had, moreover, crystallized in the bottle which contained the liquid. Its specific gravity was 1280. One thousand parts of the dry salt contained of

Muriate of magnesia	640
Sulphate of magnesia	260
Muriate of soda	100
	1000

Pan scale from Lymington.

8. The pan-scale from Lymington contained

Muriate of magnesia	29
Dessicated sulphate of magnesia	18
Carbonates of lime and magnesia*............	127
Sulphate of lime........	216
Muriate of soda	610
	1000

No sulphate of soda present.

From the very near approximation of the proportions between the sulphate of barytes and ammoniaco-magnesian phosphate, obtained in the analysis of all these products of seawater, to those which result from the decomposition of two equal quantities of sulphate of magnesia, it may be inferred,

* The proportion of these carbonates I was prevented from determining by an accident.

that

Fig. 1.

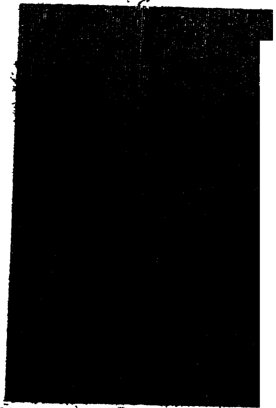


Fig. 2.

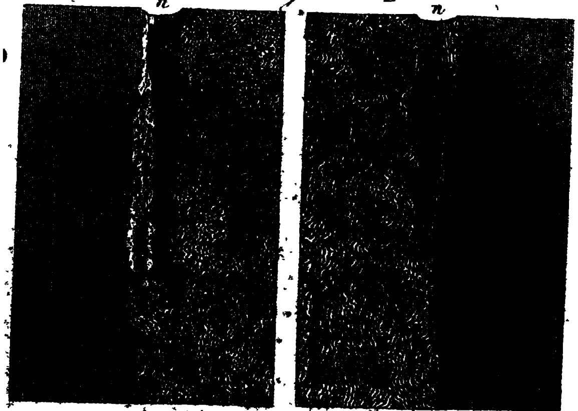
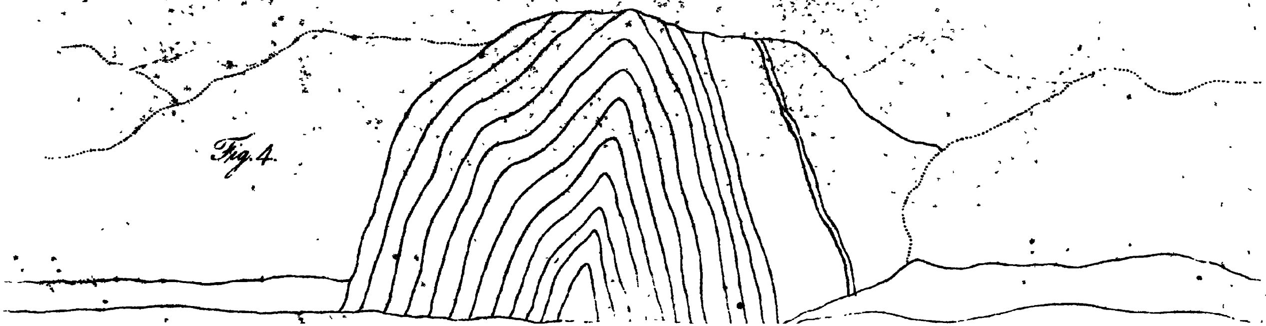


Fig. 3.

Fig. 4.



that they contain no sulphate of soda*. For example, to decide whether the Scotch salt contains an alkaline sulphate, or not, I dissolved 1500 grains in a pint of boiling water, and evaporated till fourteen drachm measures only remained, the common salt being removed as soon as it was formed. The residuary liquid was divided into two equal portions, one of which gave 18½ grains of sulphate of barytes, and the other, 14 grains of ammoniaco-magnesian phosphate. The proportion between these numbers is so nearly that which has been already assigned, (viz. 112 to 90,) that we may safely infer the total absence of sulphate of soda. This salt, indeed, is considered as incompatible with muriate of magnesia; but after digesting, for two or three days, 100 grains of the former, with 20 of the latter, evaporating to dryness, and washing the residuum with repeated effusions of alcohol, I found, that two grains of the muriate of magnesia had escaped decomposition.

IV.

*On the Action of the Electric Fluid, by which an Iron Cylinder an Inch and half thick was torn asunder· in a Letter from Mr.*** to* J. C. DELAMETHERIE†.

SIR,

I Have already communicated to you, in different letters, my experiments on various metallic cylinders, that were torn by electric discharges. I half filled with water the cylinder, Plate VII, fig. 1, which was open only at one end. In this water I immersed a small leaden rod, which I surrounded with wax at the place where it entered the cylinder, in order to insulate it, as at fig. 3. I then formed a communication between one of the surfaces of a powerful elec-

Metallic cylinders burst by electric shocks.

* I employed more attention in investigating the presence of sulphate of soda in the products of seawater; because this salt is stated to be one of its ingredients by the Bishop of Llandaff, (Chemistry vol. ii. p. 62,) and by other chemical writers.

† Journ. de Physique, vol. LXVIII, p. 411.

tric

Explosions.

tric battery and the bottom of the cylinder, and between the other and the leaden rod. The explosions were very strong; the water being frequently ejected, with violence, and the leaden rod thrown to a distance. After several explosions the cylinder was more or less torn.

Application to natural phenomena.

Reading in your Journal for january, 1809, the reflections, with which you conclude the article of earthquakes, and the influence that water penetrating through the clefts of the mountains must have on them, I considered the electric action of our apparatuses as an imitation of what passes in the mountains where these phenomena take place. The electric fluid acts in its usual manner on the inflammable substances contained in them, and produces on them considerable effects. My preceding experiments had shown, that this fluid could burst metallic cylinders of considerable strength; and I was willing to try, whether it could not produce still greater effects. Accordingly I made a cylinder of iron, fig. 1, twenty-seven lines high*, and eighteen in diameter; with a hole in the centre, *n*, 1¼ line in diameter, and 18 deep. It was subjected to the same experiments as the former cylinders, and was burst by seventy explosions.

Experiment with an iron cylinder, near three quarters of an inch thick.

Battery of 100 feet

The battery I employed had 100 feet of metallic coating. At every discharge the opening was well cleaned with an iron wire and water, let fall into it drop by drop, to remove all the oxide of lead and iron, which was separated at each discharge in half fused and oxided grains. Then, having emptied out the water by shaking the cylinder, and filling it with fresh water, the wires ready prepared being placed in the opening, the little apparatus was placed in a box, and, Henley's electrometer indicating an intensity of 60° or 65°, the discharge was made. Seventy discharges produced the rent *a*, fig. 1. It required about a quarter of an hour to charge the battery; so that two intelligent workmen could obtain about twenty discharges a day, even in my absence.

burst it in 70 discharges.

I beg you to get the cylinder sawn in a direction opposite to the rent *a*, till you come to the hole *n*, in order to observe the effects of this rupture within†.

In

* French measure. The dimensions are the same in the plate. C.

Appearance of

† I requested Mr. Dumoutiez, who is well experienced in the construction

In my former experiments I had observed, that, when the electric fluid was constantly directed to one point, the metallic decomposition sooner took place. In consequence I took care to make the cylinder rest on its centre, raising the metallic plate a little at a single point, and surrounding this point with wax. The effect was so striking, that at the 70th discharge the opening was in the state represented at fig. 1 and 2.

Strongest effect when the fluid is directed to one point.

Can we now question the burning of metals interiorly by the passage of the electric fluid, which some philosophers have long ago considered as endued with acid properties?

Metals may be burned by the electric fluid

Let my experiments be repeated in vacuo by a simple stream of the fluid, and it will be found, that the oxided and fuliginous products will be equal to those obtained in atmospheric air.

in vacuo.

Let the experiment afterward be made in water, with short wires in a small tube, and very long ones in another tube; it will be found perhaps (but this requires farther examination) that the oxide precipitated at the place where a large wire comes out, as those of small tubes of silver, or of an alloy of gold and silver, will be less in quantity than in the tube four or five feet long, the positive wire of which is three fourths the length of the tube, and the negative wire one fourth. My friends form the same judgment in this respect. The oxided product was so copious, after the continued action of the electric current for five or six hours, that it covered the bottom to the extent of more than ten lines toward the apparatus; the black matter,

Effects of the experiment in water.

struction of philosophical instruments, to saw the cylinder as the author requested. An interior view of the cylinder, when thus sawn asunder, is given at fig. 2 All the part, from the outer crack *a* to the centre, and a little beyond it, is torn; and in several places exhibits the same appearances as a broken iron bar, in others laminæ with a resemblance of crystallization, unless you would rather consider them as the effect of iron of a bad quality. The lower part of the cylinder, from the bottom of the hole *n*, is equally torn. A portion of the torn part appears to be oxided. The small detached pieces appear also to be oxided.

the cylinder.

The author has since sent me three other cylindrical pieces, one of which has an opening of six lines. They have all been burst by repeated electrical discharges, but I have not sawn them asunder. *J. C. Delamétherie.*

Others burst in the same manner.

which

which is a mixture of carbon and hidrogen, rendered the negative wire black for more than two inches; the water and the rest of the wire were tinged yellow; and when I made the discharge, this tinge spread more than a foot round the long wire, now become negative. All the rest of the water remained limpid.

Theory of two fluids.

When I first began these researches, I attempted to explain the phenomena of electricity by Symmer's theory of two fluids, because this theory is in no respect inconsistent with elective attraction, to which I have ever considered all substances obedient. But the moment I succeeded in detecting the passage of the fluid through the pores of uncoated glass, I confessed my mistake. I am ready to do the same, with respect to the inferences I draw from my last experiments, and I shall hereafter follow your advice, "seek for new facts, and let theories spring from them as they may."

Was not Mr. Orsted right in saying, that, when the electric fluid was better known, we should be able to explain several natural phenomena, hitherto inexplicable?

V.

Observations on the preceding Experiments. By J. C. Delametherie.

The four new planets fragments of a large one.

Do not these effects of electricity, in rupturing masses of so much tenacity as iron cylinders, give some probability to the idea of those German astronomers, who have said, that the four new planets, Ceres, Juno, Pallas, and Vesta, are fragments of a larger planet formerly situate between Mars and Jupiter, and broken by some unknown cause? Suppose, for instance, that the centre of this planet was a mass of metal, similarly circumstanced with the author's cylinders; and that a metallic vein, or any other conducting substance, acted like the leaden wire, and conducted the electricity of the atmosphere into the metallic mass: might not a great number of strong discharges, such as occur in violent thunderstorms, burst this metallic mass asunder, and project

ject the different parts to a distance, as the little bit of lead in the author's valuable experiments?

At least there can be no doubt, that these explosions, in our thunderstorms, must produce effects more or less considerable in the heart of our globe, as I have shown in my Theory of the Earth, vol. III, p. 224.

Thunderstorms must act on the substance of our globe.

The terraqueous globe, I there observed, is commonly considered as a vast storehouse of the electric fluid. Natural philosophers call it the common reservoir. This supposes all the bodies, that compose the globe, to be in a state of habitual electricity; a principle admitted by all philosophers.

Theory of the Action of electricity on the Earth.

But this electricity is not always the same, either with respect to the whole body of the globe, or to its different parts.

It is completely demonstrated, that there is a reciprocal communication between the electricity of the atmosphere, and that of the globe. We must therefore consider the globe and its atmosphere as two electrical bodies, each charged with its natural electricity: and as these two bodies are in contact, their electricity should be in equilibrium according to their natural capacity, so that one has not more electricity than the other.

But local circumstances may increase the electricity of one of these bodies in certain places, and diminish it in others. The equilibrium will then be disturbed, and the electric fluid will rush from the positive body to the negative. This happens in the case of lightning, whether the stroke be descending or ascending.

When a portion of the atmosphere is positively electrified with respect to the terrestrial bodies opposite to it, the electric fluid rushes into these bodies, as soon as the distance allows. This communication is effected rapidly and with explosion, in certain circumstances; which constitutes the descending thunderstroke. On the contrary it is effected slowly and insensibly, if there be points to draw of the electric fluid gradually, or if it be conveyed by rain, dew, &c.

If the atmosphere be negatively electrified with respect to the opposite terrestrial bodies, it will attract their electric fluid. This will constitute the ascending stroke, if it be effected

fected with explosion. Otherwise the communication will take place slowly and gradually.

Let us suppose, that the portion of the atmosphere over a lofty peak, as Mount Blanc, be electrified positively with respect to it; that is, contains more of the electric fluid: it will communicate its electricity to the mountain, either by flashes of lightning*, or slowly and gradually, and this electricity will pass from this lofty peak to the adjacent parts.

Let us suppose on the contrary a mountain like Etna, the vapours of which, ascending constantly from its internal fires, are loaded with positive electricity, which they take from the bosom of the mountain: the body of this peak must be in an habitual state of negative electricity, with respect to the parts of the earth contiguous to it.

VI.

Arrangement of the Strata of the Hill of Durbuy, in the Department of the Sambre and Meuse: *by* J. J. Omalius de Halloy *.

Arrangement of strata important in geology.

THE examination of the different arrangement of the strata that constitute the globe is one of the most interesting points of geology. Indeed till the existence of strata was suspected, this science did not begin to free itself from the chimerical forms, in which it was retained by absurd hypotheses framed on metaphysical abstractions, and take its place among the physical sciences founded on observation. The great book of Nature cannot be too frequently consulted; but as the ablest men have it not in their power to turn over all its pages, part of the task must devolve to subordinate labourers, who note down every thing that appears to them worthy notice, at the hazard of busying themselves about things of little importance.

Precipitates arrange them-

In the present state of things, every substance precipitated from a fluid in successive periods will arrange itself in

* Stones vitrified by flashes of lightning are found on Mount Blanc.

* Journal des Mines, vol. XXI, p 475

in horizontal strata: and what we know of the laws of gravitation does not allow us to conceive the possibility of a precipitate, the particles of which are not yet held together by the force of cohesion, supporting itself in vertical, or even inclined strata. Yet this position exists in many portions of the Earth, particularly those of a certain age. selves horizontally. Strata not so.

It was natural, that various hypotheses should be formed, to account for such a singular fact. The opinion that appears best founded, and most generally adopted, is that of the sinking in of certain parts of the Earth. I do not mean to controvert an hypothesis adopted by many learned men, but I shall submit to them a circumstance, which I cannot explain.

The Ourthe, which flows through the department of that name, and that of the Sambre and Meuse, traverses a narrow valley, bordered by lofty hills, several of which are perpendicular. The mineral strata that compose these hills are all more or less inclined, and sometimes in a direction different from that of the valley. This takes place particularly at Durbuy, a small town in the third circle of the department, where the hill is formed of a bluish bituminiferous carbonate of lime, that constitutes several systems of strata. One of these systems is cut by a perpendicular plane, so that the remarkable arrangement of its strata is easily seen. See Plate VII, fig. 4. They have an inclination of about 60° or 80°, and are placed one upon another, like a series of pairs of rafters laid one upon another. The visible part of the first stratum in the centre exhibits only the shape of a wedge. On the top and sides of this stratum is placed a second, the summit of which is equally cuneiform; while the sides, sloping like those of a roof, cover the first wedge. Thus they continue in succession to the top of the hill. But the hill has been flattened by some cause or other, and the last strata have not the same summit as the rest, as they are merely applied on each side of the preceding, sloping in different directions, but not united at the top*. Some in different parts of France. Remarkable one.

What

* It is very probable, that, if the uppe[r] surface of the hi[ll] were examined attentively, the exterior strata wo[u]ld be found to unite in their prolongation,

The strata thickest where they might have been expected to be thinnest.

What appears to me worthy of attention is the solid cuneiform summit, that unites two inclining sides, which are near 100 met. [100 yards] high from the river, and we know not how deep they descend. The particles that compose this summit are as intimately united as those of the rest of the stratum: there is no perceptible joint, no regular fissure, indicating the strain experienced by these strata in bending, if they were originally deposited in a horizontal situation: and supposing the calcareous matter to have been sufficiently soft when this happened, to bend without cracking, I cannot conceive what cause could produce a nearly pointed summit, resembling rafters united by a sloping cut, the obvious effect of which is an increase of thickness, while a simple bend necessarily diminishes it. Can the different velocities of the masses, combined with the pressure of the adjacent parts, which so happily account for strata bent on a small scale, apply equally to an entire hill, bent in the great, if the expression may be allowed; and the summit of which is not overtopped by other mountains*?

Geology of the neighbouring country.

These observations lead me to say a few words on the geological constitution of that portion of the departments of the Sambre and Meuse and of the Ourthe, which is included between the Meuse, the Lesse, and the Ourthe. The strata, that form the soil of this country, exhibit every possible variety of inclination. They not only vary from horizontal to perpendicular, but we every instant meet with curvatures or folds. Every thing indicates prompt and violent catastrophes. The direction† of these strata too

prolongation, like those they cover; or that they would diverge, and the angular bend that unites them disappear at some distance from the precipice: for we can scarcely suppose, that the surface terminating all these strata at the top of the hill is exactly parallel to the line of junction of the central strata, and that it retains this parallelism throughout their whole extent. *Note of the French editors.*

* For some other singular arrangements of strata, see Journal, vol. XX, pages 62 and 64, and Plate II.

† I make a distinction between the direction and inclination of the strata: the latter is indicated by the angle, which the horizon forms with the lower plane of the stratum, while the direction is the common section of this plane with that of the horizon.

presents

presents much irregularity in its minuter parts. But amid so much confusion we observe a pretty constant direction from south-west by south to north-east by north, making an angle of about 35° with the meridian. This situation of the strata coincides with the aspect of the country, divided into long hills, and narrow valleys, lying in the same direction. But these valleys are not the only ones, that furrow the country; for the hills are frequently intersected by irregular valleys, commonly serving as a passage for the rivers.

Does not this agreement of the longitudinal valleys with the direction of the strata lead to the supposition, that their origin is connected with the circumstances that formed the strata, or gave them their inclination? and the irregularity of the tranverse valleys, and their agreement with the present course of the waters, that they are owing to subsequent events, produced by the course of some fluid? Inferences.

I shall conclude with an observation, the explanation of which appears also to depend on hypotheses relative to the causes of the inclination.

All the strata of this country are composed in general of gritstone, schists, and bitumeniferous carbonate of lime, the reciprocal superposition of which announces a contemporary formation*. Yet we may observe in an infinite number of instances, that the plains, or summits of the hills, exhibit only gritstone or schist, while the declivities present these strata alternating with limestone; and that the latter substance alone forms the bottom of most of the longitudinal valleys†. I do not think this effect can be Alternation of gritstone, schist, and limestone.

* All these are of secondary formation, and contain fragments of organized substances, such as vegetables, and animals without vertebræ.

† In these valleys are found many blackish pebbles, which I consider as real quartz-agates (silex), possessing all the outer characters of the *kiesel-schiefer* of the German mineralogists. This substance indeed, like all the quartzes, is infusible, and Wiedenmann speaks of the fusibility of the kiesel-schiefer. But may not this property be ascribed to the accidental admixture of some calcareous particles? an opinion the more probable, as our black quartzes are frequently found in nodules in carbonate of lime. Pebbles.

ascribed

ascribed to erosion by water; for in the transverse valleys the limestone appears most unalterable. While the acclivities of schist and gritstone are covered with vegetation, those of limestone exhibit perpendicular sections and sharp ridges.

VII.

Method of stabbing Hoven Cattle, to discharge the Rarified Air from the Stomach, when they have been overfed with moist Clover Grass: communicated by Mr. W. WALLIS MASON, *of Goodrest Lodge, near Warwick**.

GENTLEMEN,

Instrument for relieving hoven cattle.

I Beg leave to lay before you a trocar and canula, for the relief of cattle, when gorged or hoven. Since I have introduced it, it has been used with the greatest success, having, in every instance tried, been proved a safe, easy, and effectual remedy.

Many annually lost by this affection.

I consider it will not be necessary for me to detail the dangerous consequences arising from cattle being hoven, as it is well known, that the public are annually deprived of numbers of valuable cattle by this disorder. I am inclined to offer it as an instrument superior to that, for which the Society granted a premium in the year 1796; as I am of opinion, that flexible tubes may be forced down the passage, which conducts to the lungs, by which most dangerous consequences would ensue. An instance of this kind occurred last year in this neighbourhood, when intending to force the passage of the paunch, and occasioned the loss of the animal.

Inconveniences of a former instrument.

Neither the farmer nor bailiff can be expected when going the rounds of the farm, to carry with him at all times an instrument so large as one of the flexible tubes; even if he had it, he could not make use of it without the assistance of a second person, and the disorder would be fatal in most instances, before such assistance could be procured.

* Trans. of the Soc. of Arts, vol XXVI, p. 128. The silver medal was voted to Mr. Mason for this communication.

I considered,

I considered, that the trocar and canula commonly used by surgeons might be employed to advantage for the relief of hoven cattle. I have improved the instrument, to answer better the purpose here intended of penetrating the tense hides of cattle; and such alteration materially facilitates the operation. Trocar and canula preferable.

The method of applying it is, to penetrate with the trocar and canula through the hide of the beast to the paunch on the near side, about six inches from the back-bone, at an equal distance from the last rib and from the hip-bone; then to withdraw the trocar, and to leave the canula in the wound, until the air which the paunch contained has escaped. The canula may then be taken out, and the wound covered with a plaster of common pitch, spread on brown paper, about the size of a crown piece. All the danger incidental to the common mode of stabbing with the knife is effectually prevented, by the canula being left in the incision when the trocar is withdrawn. Method of using them.

The small expense of the instrument, its portability, the ease with which it can be used by an individual, its safety and efficacy in use, as it has not in any instance failed of complete success, will, I hope, be sufficiently evident, to recommend it to the attention of the Society. A great saving would arise to the owners of cattle, and to the country at large, from general adoption of its use. Their advantages.

A feeding ox will thrive as well after the operation, as if it had never been affected by the disorder. Cows in calf are in no danger from its use. It has been found particularly beneficial in preserving rearing calves, and young cattle, when afflicted with this disorder, which had heretofore been fatal to great numbers of them.

I beg leave to add the certificates of a few of those gentlemen who have witnessed the utility of this method, and whose recommendations have stimulated me to submit it to the Society, in hopes, that by their liberal patronage it may be rendered more generally beneficial to the public.

I have the honour to remain, Gentlemen,

Your obedient servant.

W. WALLIS MASON.

Certificates

Certificates.

Certificates from the following gentlemen testified, that they had experienced the efficacy of Mr. Mason's trocar, had proved the safety of the operation, and the instantaneous relief which it had never failed to produce without leaving any blemish, or dangerous consequence, from its application.

JOHN FORD NAISH, Leek Wooton.
THOMAS BRYAN, Warwick.
WILLIAM ORAM, Warwick.
WILLIAM LEDBROOKE, Northend.
RICHARD CATTELL, Milverton.

Reference to the Engraving of Mr. WALLIS MASON'S *Trocar and Canula.*

Instrument described.

FIG. 1, Pl. VIII, is a representation of Mr. Wallis Mason's trocar and canula. The blade of the trocar is of steel *a a*, fig. 4, fixed into the wooden handle *b b*. The shape of the blade of the trocar is oval, as shown in the end view of the canula, fig. 3. The canula or sheath *e e*, figs. 1 and 2, is an oval tube, which exactly fits the blade of the trocar; *f f* is a concave circular plate, fixed at the end of the canula, forming a hilt, to prevent the instrument from giving too deep a wound when used; the end *g* of the canula is worked down to a sharp edge, that it may not obstruct the passage of the instrument. The drawings are on a scale of one inch to two inches and a half; in figs. 1 and 2, the trocar and canula are shown edgeways, or in the shortest diameter of the ellipsis; in fig. 4, the trocar is shown flat, in its longest diameter.

VIII.

Description of a Swivel-headed Churn Staff, to facilitate the making of butter: by Mr. TIMOTHY FISHER, *of Ormskirk, Lancashire*.*

SIR,

Swivelheaded churnstaff.

I Beg leave to lay before the Society of Arts &c. my swivel-headed churn staff, which, on repeated trials, is now fully

* Trans. of the Soc of Arts, vol XXVI, p. 181. Five guineas were voted to Mr. Fisher for this invention.

fully proved to answer the very desirable purpose of relieving the hard labour of churning; which it does in a much greater degree than could be supposed, from a slight view of its simplicity and apparent small deviation from the common churn staff. It however passes with much more ease through the cream. It must be worked much slower than the common, otherwise it is found to churn the cream too soon, or, according to the technical term in this county, *to swell it*.

Requires to be worked slowly.

I have tried it in a variety of forms and sizes; with six wings the labour was less relieved; also when I gave less bevel to the ends of the wings.

When I gave more bevel it passed through without producing the intended effects. Experience therefore has convinced me, that it is best to have four wings from six to seven inches in length, from the centre, according to the size of the churn for which it is intended, from two and a half to three inches in breadth, made plane in the centre or middle, about the fourth part of their length, and then bevelled regularly off, so that the extreme point shall form an angle of about 45 degrees with the plane of the middle. The plane part acts with its usual force upon the middle of the body of the milk; and the points turning rapidly round give a kind of compound motion to the whole, and that also alternate, and yet it does not in the least splash or throw out the cream as in the common mode.

Best form of it.

I am, Sir,

Your obedient servant,

TIMOTHY FISHER,

Gun-maker.

Thomas Ecclestone, Esq., of Scarisbrick Hall, near Ormskirk, certified, that he had seen Mr. Fisher's new method of churning butter, and that he thinks it superior to any he had heretofore known for that purpose, and that such was also the opinion of several other persons in the farming line who had witnessed its effects.

Its superiority.

Reference to the Engraving of Mr. Fisher's Churn Staff.

Fig. 7, Pl. VIII, is a section of an upright churn, in the situation it would be when at work, and figs. 5 and 6 are enlarged

Explanation of the plate.

larged views of the head of the churn staff. ABDE fig. 7, is a section of the churn; FG is the lid; KL is the churn staff, and HI the wings, or beaters; it is this part only which differs from the ordinary churn; it consists of four wings or vanes MNOP, fig. 5, firmly fixed together, and turning freely on a pin driven into the end of the churn staff. The flat part of each vane is cut, so as to be inclined to the plane in which all four lie, in the same manner as the sails of a windmill, as is well explained by figs. 5 and 6. When the beater is moved up and down through the cream, its action upon the oblique vanes causes it to turn round upon a pin above mentioned, as a centre. *a*, Fig. 7, is a small wooden bolt sliding in a groove made in the churn staff, KL; its end shuts into a hole *b*, fig. 5, made in one of the vanes; when this bolt is pushed down, it prevents the vanes from turning round, for the purpose of collecting the butter together at the top of the butter-milk when the churning is done.

IX.

Improvement in Cutting Silk Shag Edgings: by Mr. PETER TANSLEY, *at the White Horse, Wheeler Street, Spitalfields**.

SIR,

Narrow shag tedious to cut with the common implement.

IN the latter end of March, 1806, my employer, Mr. Jeffery of Bow Lane, Cheapside, applied to me with a pattern of silk shag, about six eighths of an inch wide, which he wished to be executed as quick as possible.

Being aware how tedious an operation it was to cut the shag with the trivat, the instrument generally used by the shag weavers for that purpose, I thought I could invent an instrument, which would cut it in one third of the usual time; I therefore drew the plan of one, and gave it to Mr.

* Trans. of the Soc. of Arts, vol. XXVI, p. 194. Five guineas were voted to Mr. Tansley for this invention.

Baker

Baker, a cutler in Artillery Passage, Artillery Street, Spitalfields, to make according to my instructions, which he did, and which answered my expectations in the work. Another therefore invented.

In October, 1806, I made for the same gentleman a figured shag, one inch and three quarters wide, and cut it with the same instrument, as certified by my employer and his son in law. I hope the Gentlemen of the Society will not think me too presuming in offering this improvement to their notice.

I am, Sir, with great respect,

Your obedient Servant,

PETER TANSLEY.

Reference to the Engraving of Mr. PETER TANSLEY'S *Implement for Cutting Shag. See Plate VIII, Figs.* 8. 9.

Fig. 8 shows the steel implement or cutter, as placed or woven within the threads, previous to their being cut with it, the cutting being performed by merely drawing out the knife, the sharp edge of which cuts the threads, and forms the shag by that operation.

Fig. 9 shows the implement detached from the work, R being the part held in the hand, and S the sharp blade which cuts the threads.

SIR,

This is to certify, that Peter Tansley, of Wheeler Street, s the sol e inventor of the cutter for making narrow shag; that its utility is well known; and that I have, within fourteen days, made four hundred of these improved instruments.

W. BAKER.

Certificates were received from several other persons, stating the very great utility of the instrument, their persuasion of its being superior to any other thing of the kind ever invented, as making the work equally good, with greater ease and in half the time, and that Mr. Peter Tansley is the original inventor.

IX.

X.

Observations on the Combustion of several Sorts of Charcoal, and on Hidrogen Gas: by THEODORE DE SAUSSURE.

(Concluded from p. 176.)

Combustion of plumbago from Cornwall.

Plumbago from Cornwall, containing 0·04 of iron, burned in oxigen.

I Burned in oxigen gas 0·588 of a gramme [9·079 grs.] of plumbago dried at a red heat. The combustion continued an hour, and left, as a residuum 0·033 of a gr. [0·51 of a gr.] of red oxide of iron. This compound, which is formed during the operation *, would consist, according to Bucholz †, of 0·0231 of a gr. [0·357 of a gr.] of iron, and 0·0099 of a gr. [0·153 of a gr.] of oxigen. One hundred parts of this plumbago therefore contain 4 parts of iron; and I burned 0·588 — 0·0231 = 0·5649 of a gr. [8·722 grs.] of carbon.

Results.

The gas contained in the receiver occupied in the shade, previous to the combustion 1894·3 cent. cub. [1153·5 cub. inch.]; thermom. at 23·12° [73·62° F.]; barometer, reduced to the temp. of 12·5° [54·5° F.], 0·7329 of a met. [28·83 inch.].

Two hours after the combustion the gas occupied 1899·3 cent. cub. [1156·57 cub. inch.]; thermom. at 25° [77° F.]; barom. corrected at 0·7329 of a met. [28·83 inch.].

Reducing the volumes of gas, before and after combustion, to the mean temperature of 12·5° [54·5° F.], and pressure 0·75796 of a met. [29·82 inch.], we find that the gas occupied,

Before combustion,	1758·8 cent. cub.	=	1071·022 cub. inch.
After combustion,	1750·4	=	1065·907
Diminution, ...	8·4	=	5·115

* Messrs. Allen and Pepys, in making the same experiment, consider the oxide of iron as completely formed in the plumbago before the combustion. This must occasion some slight difference in the results of their operation.

† Ann. de Chim. vol. LXV, p. 202: or Journal, vol. XXV, p. 353.

The

The plumbago emitted neither vapour nor smoke in burning. The muriate of lime placed in the receiver, and which was always weighed in a closed phial, acquired an increase of 5 cent. [0·772 of a gr.]: but I learned from a comparative experiment, that it absorbed a centigramme [0·154 of a gr.] of water from the atmospheric air during the time of its being put into the receiver and taken out again. The gas employed for the combustion contained no visible water, but it was in a state of extreme humidity; and the hygrometrical water in it, at the temperature of 25° [77° F.], weighed 3·9 cent. [0·6 of a gr.]. As the muriate of lime must have acquired 4·9 cent. [0·754 of a gr.] by these two additions of weight, it does not appear, that the plumbago produced any sensible quantity of water in burning.

No smoke, or vapour, emitted; and no water produced.

The hidrosulphuret of potash indicated 189·75 parts of oxigen gas, and 10·25 of nitrogen, in 200 of the impure oxigen gas, which the receiver contained before the combustion. Potash detected no acid gas in it. 200 other parts of the same gas, mixed with 400 of hidrogen gas, were reduced by detonation to 33; and consequently contained 189 oxigen and 11 nitrogen.

State of the gas employed.

From 100 parts of the gas left after the combustion of the plumbago potash absorbed 63·42 of carbonic acid gas

Carbonic acid formed.

After the separation of this acid gas, I examined whether there were any hidrogen gas in the residuum, by detonating it with a mixture of equal parts of hidrogen and oxigen, and treating with potash the gas remaining after the detonation. These operations showed me, that the plumbago had given out no hidrogen.

No hidrogen evolved.

The hidrosulphuret of potash indicated in 100 parts of the gas, which the receiver contained after the combustion of the plumbago [and abstraction of the carbonic acid], 87 parts of oxigen gas, and 13 parts of nitrogen. Another 100 parts were mixed with 200 of hidrogen gas, and reduced to 40 by detonation. These 100 parts therefore contained 86·66 of oxigen, and 13·34 of nitrogen. According to the process with hidrosulphuret, the 1750·4 cub. cent. of gas found in the receiver after the combustion of the plumbago contained

Carbonic

State of the gas after the process.

Carbonic acid gas	1110·1 cent. cub.	676 cub. inch.
Oxigen gas	557.06	339·22
Nitrogen gas	83·24	50·68
	1750·4	1065·9

Composition of carbonic acid gas. If we calculate the composition of the carbonic acid gas from the quantity of oxigen employed to form it, we find, that the oxigen gas which disappeared in this process was 1669·8 — 557·1 = 1112·7 cent. cub. And deducting 7·3 cent. for the weight of 0·0099 of a gr. of oxigen, that oxided the iron in the plumbago, there will remain 1105·4 cent. cub. [673·13 cub. in.] of oxigen gas, that entered into the composition of 1110·1 cent. cub. [676 cub. in.] of carbonic acid gas. If instead of the measures of these gasses we substitute their respective weights, we find by the rule of proportion, that 100 parts of carbonic acid gas by weight contain 72·64 of oxigen, and 27·36 of carbon.

100 parts contain 27·36

If we calculate the composition of the acid gas from the weight of the plumbago burned, we find, that 0·5649 of a gr. [8·722 grs.] of plumbago (deducting the iron contained in it) were employed to form 1110·1 cent. cub. [676 cub. in.], or 2·0621 gr. [31·839 grs.] of carbonic acid gas. Consequently 100 parts of this acid by weight contain 27·39 parts of carbon, and 72·61 of oxigen.

or 27·39 of carbon.

2d Experiment on the Combustion of Plumbago from Cornwall.

2d Exp. on plumbago. As plumbago was the only carbonaceous substance, of all I tried, that yielded neither water nor hidrogen in burning, I thought it necessary, to repeat the process.

Results. The results were nearly the same as before. The composition of the carbonic acid gas came out 27·381 carbon, and 72·619 oxigen, calculating from the weight of plumbago burned; and 27·04 of carbon, 72·96 of oxigen, calculating from the oxigen gas consumed.

27·38 or 27·04 per cent of carbon.

Diminution of the gas. Two other trials were made, and in all there was a little diminution of the oxigen by burning, owing chiefly to the oxidation of the iron mixed with the plumbago.

Combustion

Combustion of Anthracite.

I burned in oxigen gas 0·549 of a gr. [8·476 grs.] of anthracite dried at a red heat*. The combustible that disappeared in this operation weighed 0·524 of a gr. [8·09 grs.], deducting the ashes, which weighed 0·025 of a gr. [0·368 of a gr.] Stone coal burned.

The receiver, in which the combustion was effected, contained previous to it 1886·4 cent. cub. [1148·7 cub. in.] of impure oxigen gas, at 0·73089 met. [28·755 in.] of the barom., and 21·56° of the therm. [70·8° F.]

After the combustion the gas occupied the space of 1894·3 cent. cub. [1153·5 cub. in.], at the same pressure, but at the temperature of 24° [75·2°].

Reducing these quantities of gas to the mean temperature of 12·5° [54·5° F.], and pressure 0·75796 met. [29·82 inches], we find, that the volume of gas was

Diminution of the gas.

Before the combustion ··	1755 cent. cub.	=	1068·7 cub. in.
After the combustion ··	1748·2	=	1064·56
Diminution	6·8	=	4·14

The anthracite formed a little smoke in burning. The muriate of lime increased in weight 8 cent. [1·235 gr.]; but as the hygrometrical water of the oxigen gas weighed 0·0336 of a gr. [0·519 of a gr.] at the temperature of 22° [71·6° F.], at which the process was conducted; and as the salt absorbed 1 cent. [0·154 of a gr.] of water from the air, during the time of arranging the apparatus; I found, that the quantity of water produced by the combustion of the coal was about 0·0364 of a gr. [0·562 of a gr.] Smoke. Water formed.

In 100 parts of the gas employed for the combustion the hidrosulphuret of potash indicated 94 of oxigen and 6 of nitrogen; Volta's eudiometer, 94·25 of oxigen, and 5·75 of nitrogen. State of the gas before the combustion.

* This anthracite is found in nodules in a transition pudding-stone, near Martigny, in Vallais. It loses by incandescence 0·16 of its weight, and then attracts a little moisture from the air. For this reason I did not leave it exposed to the air after drying. On cooling it absorbed its own bulk of atmospheric air. This absorption may be neglected.

nitrogen. Hence the 1755 cent. cub. of gas employed in the combustion contained 100·9 of nitrogen.

State after it. The 1748·2 cent. cub. of gas in the receiver after the combustion were found to consist of

Carbonic acid gas ....998·27 cent. cub.	= 607·9 cub. inch.
Oxigen gas ....616·82	= 375·61
Nitrogen gas ....100·9	= 61·44
Oxicarburetted hidrog. 32·21	= 19·61
1748·20	= 1064·56

Combustion of the Coal formed by Oil of Rosemary.

Charcoal from essential oil. By passing oil of rosemary in a state of vapour through a red hot porcelain tube, I obtained a coal, which appeared well adapted for determining the composition of carbonic acid.

Its properties. The coal, heated red hot in the fire, and extinguished without the contact of air, did not absorb different gasses, in which it was immersed. It did not increase in weight after this incandescence, even though exposed several months to the open air. It formed no ashes.* It burned with the same difficulty as plumbago. It was heavy, and sufficiently hard to scratch glass.

Burned. I burned 0·513 of a gr. [7·92 grs.] of this coal in a receiver containing 1947·93 cent. cub. [1186·19 cub. in.] of oxigen gas at 26·87° [80·37° F.] temperature, and 0·73684 met. [[illegible] inches] pressure.

Results. After the combustion the gas occupied the space of 1958·83 cent. cub. [1192·83 cub. in.], at 27·5° [81·5° F.] temperature, and pressure as before.

On reducing the volume of the gas before and after the experiment to the mean temperature of 12·5° [54·5° F.], and pressure of 0·758 met. [29·82 in.], we find it occupied

Before combustion,	1791·5 cent. cub.	= 1090·93 cub. in.
After	1797·2	= 1094·4
[illegible]se.......	5·7	= 3·47

No

No visible vapour or smoke was formed during the process. The muriate of lime suspended in the receiver acquired an addition of 5 cent. [0·772 of a gr.]; but I have already observed, that this would result from the water contained in the oxigen gas, and in the atmospheric air to which it was a few moments exposed. No smoke emitted. No water formed.

The gas in the receiver after the combustion consisted of State of the gas.

Carbonic acid gas····	1008·9 cent. cub.	= 614·37
Oxigen gas ········	508·5	= 309·65
Nitrogen, and oxicarburetted hidrogen *	279·8	= 170·38
	1797·2	= 1094·4

On calculating the composition of the carbonic acid gas from the weight of coal burned, it comes out 27·109 of carbon and 72·891 of oxigen; and from the oxigen consumed, 72·88 oxigen and 27·12 carbon. 100 parts carbonic acid contain 27·109 or 27 12 of base.

Combustion of Box Charcoal.

I exposed some charcoal, made from box, fastened on a plate of platina by wires of the same metal, to a long-continued red heat in a close vessel. This charcoal was weighed in a close vessel immediately after cooling, and was not afterward exposed to the air longer than was necessary for putting it into the receiver, where it was to be burned. When this combustible in a state of incandescence was immersed in mercury, cooled there, and passed through it into a jar full of common air, this air underwent in a few instants a diminution equal to seven times the bulk of the charcoal. In the present experiment this charcoal weighed 0·591 of a gr. [9·125 grs.], displaced 0·95 of a cent. cub. [0·579 of a cub. in.], and consequently contained 6·65 cent. cub. [4·05 cub. in.] of atmospheric air. After combustion in oxigen gas it left a residuum of 2 cent. [0·31 of a gr.] Box charcoal burned. Absorbs 7 times its bulk of air presently.

* The quantity of oxicarburetted hidrogen must have been but very small, since the bulk of the nitrogen present before the combustion was 275 cent. cub [167 46 cub. in]

Increase of gas. The gas reduced to mean temperature and pressure measured

Before the combustion,	1683·7 cent. cub.	= 1025·29 cub. in.
After	1709·7	= 1041·12
Increase	26	15·83

Vapour emitted. The charcoal formed a light vapour in burning. The increase of weight of the muriate of lime in the receiver, after deducting that of the water absorbed from the oxigen gas and from the atmosphere, was 2 cent. [0·31 of a gr.], Water produced. the weight of the water produced by the combustion of the charcoal.

State of the gas after combustion. The gas in the receiver after the combustion consisted of

Carbonic acid gas	1074·4 cent. cub.	= 654·26 cub. in.
Oxigen gas	555·89	= 338·51
Nitrogen gas	60·08	= 36·58
Oxicarburetted hidrogen	19·33	= 11·77
	1709·7	= 1041·12

State before. The receiver contained 56·4 cent. cub. [34·34 cub. in.] of nitrogen previous to the combustion, beside what belonged to the atmospheric air absorbed by the charcoal.

100 parts carbonic acid contain 27·15 of carbon. On calculating the composition of the carbonic acid gas from the weight of the oxigen, that disappeared during the combustion, we find it to consist of 72·85 oxigen, and 27·15 carbon. The calculation from the weight of the charcoal consumed comes very near this; but as it requires a conjectural estimation of the weight and formation of the oxicarburetted hidrogen, it can be considered only as an approximation.

This charcoal contained both oxigen and hidrogen. This experiment is not of itself sufficient to decide, whether both the elements of the water existed in the charcoal before the combustion, or the hidrogen alone: but it is to be observed, that the bulk of the carbonic acid produced was fully equal to that of the oxigen gas consumed; and, the result being the same as that furnished by the plumbago, which formed no water, we have hence reason to presume, that both the elements of this fluid existed in the charcoal.

Combustion

Combustion of Charcoal that had been employed for preparing liquid hidrogurelted sulphur.

The great quantity of hidrogen gas that is produced, when sulphur is passed over charcoal in a red hot tube, has led to the conjecture, that this gas does not belong wholly to the sulphur; and that this process might afford the means of obtaining charcoal perfectly pure, or freed from hidrogen *. For this reason I examined the products of the combustion of some fir charcoal, which had lost half its weight in the formation of liquid sulphur; and which had been afterward exposed to a red heat in contact with air, till it appeared to be divested of sulphur, or no longer diffuse any sulphurous smell while incandescent. Immediately after cooling it was enclosed in a phial, and weighed in it. It then weighed 0·532 of a gr. The charcoal I burned absorbed in cooling $4\frac{1}{2}$ times its bulk of atmospheric air, or 0·01 of a gr.; and this being deducted, 0·522 of a gr. [8·06 grs.] remained for the true weight of the charcoal. When burned in a receiver filled with oxigen gas, it left 0·012 of a gr. [0·185 of a gr.] of ashes.

Charcoal left after the preparation of liquid sulphuretted hidrogen.

Burned in oxigen gas.

On taking out of the receiver a sufficient quantity of gas for examination, and throwing away the rest, contrary to my expectation it emitted a very striking smell of sulphurous acid gas, though the red hot charcoal had no perceptible smell. The extreme difficulty with which this combustible burns can be ascribed only to the intimate combination of the sulphur with the charcoal. Common charcoal acquires by long-continued incandescence a kind of incombustibility, but it does not come near that of sulphuretted charcoal.

Retains sulphur very strongly,

which renders it difficult of combustion, much more than long incandescence.

The gas in which this charcoal was burned consisted of

State of the gas before combustion.

Oxigen gas	1537·9 cent. cub.	=936·51 cub. in.
Nitrogen	123·3	= 75·08
	1661·2	=1011·59
Measuring after combust.	1688·2	=1028·03
Increase	27	= 16·44

Increase.

* Ann. de Chim. vol. LXI, p. 139: or Journal, vol. XVIII, p. 48.

State after combustion.

After the combustion the gas consisted of

Carbonic acid mixed with sulphurous acid gas	950·45 cent. cub.	=	578·78 cub. in.
Oxigen gas	595·06	=	362·36
Nitrogen	116·80	=	71·11
Oxicarburetted hidrogen	15·89	=	9·68
	1688·2	=	1028·03

Water produced.

From this combustion we cannot deduce any very precise conclusions respecting the composition of carbonic acid gas; since, independent of the uncertainties arising from the production of water, which amounted to 0·018 of a gr. [0·278 of a gr.], and of the oxicarburetted hidrogen, some sulphurous acid was produced, and absorbed by the potash with the carbonic acid. We may presume however, that the charcoal contained but a very small quantity of sulphur and hidrogen; for, if we were to consider the mixture of carbonic acid and sulphurous acid gas as so much pure carbonic acid gas, the results of the combustion of the sulphuretted and hidroguretted charcoal would approach very near those furnished by the carbon of plumbago.

Furnished nearly as much hidrogen as common charcoal.

The results of this combustion however show, that the charcoal employed in the formation of hidroguretted sulphur furnishes nearly as much hidrogen, as charcoal that has not been subjected to this process. Hence we may infer, that the hidrogen of the hidroguretted sulphur is not furnished by the charcoal. It is probable, that sulphur contains either water, or oxigen and hidrogen; and that the charcoal forms the hidroguretted sulphur by abstracting the hidrogen.

General results.

Recapitulation of the principal Observations.

Plumbago.

Cornish plumbago, burned in oxigen gas, yields nothing but carbonic acid gas and oxide of iron, without any mixture of water, or of hidrogen gas.

Its combustion shows, that 100 parts of plumbago contain 96 of carbon and 4 of iron; and that 100 parts by weight of carbonic acid gas contain between 27·04 and 27·38 parts of carbon. We find the same results, whether we calculate from the weight of the carbon burned, or the

quantity

quantity of oxigen consumed, supposing the carbonic acid gas to contain its own bulk of oxigen gas.

Charcoal of essential oil.

The purest charcoal I have burned, next to plumbago, is that produced by decomposing the essential oil of rosemary in a red hot tube. Probably other oils of the like kind would furnish a similar charcoal. In its combustion it did not form any notable quantity of water: but it gave out some oxicarburetted hidrogen, though in too small a quantity, for the composition of the acid gas to be sensibly modified by it. From this experiment I found, that 100 parts of carbonic acid contain 27·11 of carbon, and 72·89 of oxigen.

Stone coal.

The combustion of anthracite, previously exposed to a red heat, furnished too perceptible a quantity of water and of hidrogen, for the results of this process to be calculated with accuracy, and compared with the preceding.

Box charcoal.

The combustion of box charcoal too, dried by long incandescence, furnished an appreciable quantity of water and oxicarburetted hidrogen.

Charcoal used for preparing liquid sulphuretted hidrogen.

The combustion of charcoal, that had served for the preparation of liquid hidroguretted sulphur, produced very nearly the same quantity of water and hidrogen, as was obtained from dried charcoal, that had not been exposed to the action of sulphur. Hence we may infer, that this substance does not take from charcoal the hidrogen it contains.

Sulphur contains oxigen & hidrogen.

It is probable, that sulphur contains either water or its elements; and that charcoal occasions the sulphur to become hidroguretted by attracting the oxigen. The recent experiments of Mr. Davy on sulphur by means of Voltaic electricity leave no doubt, that this substance contains a pretty considerable quantity of oxigen and hidrogen.

Increase or diminution of the gas.

In these experiments where I burned charcoals containing hidrogen, the gas, in which the combustion was effected, was sometimes a little increased, at others a little diminished in bulk. This difference appeared to me, to depend chiefly on the more or less perfect combustion of the hidrogen evolved. This combustion was more or less complete, not only according to the proportion of circumambient oxigen gas, but to the intensity of the heat, which in my experiments varied.

XI.

XI.

On Meteorological Nomenclature, in answer to LUKE HOWARD, *Esq. By* J. BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR, *Liverpool, July* 11, 1810.

On meteorological nomenclature.

I Read with some concern, in your last number, the critique of Mr. Luke Howard on my meteorological observations. I was concerned to find, that my attempts to illustrate a part of science, upon which he has bestowed so much attention, should not have met with a more favourable reception from him; and I was still more concerned to perceive, that he was offended at me, for rejecting the nomenclature which he proposed, to express the different modifications of the clouds. I briefly explained my motives for so doing, and surely not in a way to have called for any harsh censure; but I shall now state more fully what I have done on the subject, in order that Mr. Howard may judge whether I was influenced by the desire of advancing my reputation at the expense of his, or even by the affectation of singularity.

I had long been in the habit of making accurate observations on meteorological phenomena, when Mr. Howard published his essay; and the first impression which it made upon me was a very favourable one, many of the terms appearing to be both characteristic and judicious. I regretted indeed, that they appeared to be connected with a peculiar hypothesis, to which I could not altogether assent; yet I determined to employ them in my own diary, and I made the experiment with both the desire, and the expectation of finding them materially useful to me. After giving them, however, a fair trial, I found them quite inadequate to express the different atmospherical appearances, which I thought of sufficient importance to be recorded, and, after some time, discontinued their use. Whether the defect in this case was imputable to the nomenclature itself, or to my unskilfulness,

unskilfulness, it is not for me to decide; but it was chiefly on this account, and partly from its connection with a peculiar hypothesis, that I neglected it, and not from any selfish desire "of making way for my own" in opposition to it.

On Meteorological nomenclature.

Mr. Howard refers me to an article in Dr. Rees's Cyclopedia, where I may find his ideas on the subject, detached from the theory with which they were before united, and resting on observation alone. This essay I shall not fail to take the first opportunity of perusing, and I shall probably find, that it will remove part of my objections to the new nomenclature. With respect to the other objection, arising from its want of minuteness and comprehension, I think the only method of effectually answering it will be for Mr. Howard himself to give a specimen of a diary constructed as mine is, so as to afford a complete history of the atmospherical phenomena of each day, expressed in his own language. I feel confident, that Mr. Howard will agree with me in thinking, that such a series of observations will tend very much to improve the science of meteorology; and I have no doubt, that he will have sufficient candour to acknowledge, that the diary, which he formerly furnished for the Athenæum was by no means sufficiently copious for the purpose.

I am so desirous of carrying my project into execution, that I shall be happy to enter upon any plan of cooperation, that shall be suggested by any of your correspondents, and none more so than by Mr. Howard. So far from having any partiality for my peculiar phraseology, no one can be more aware of its defects than myself, or more desirous, that it should be exchanged for a language that may be more scientific, and more correct.

I am, Sir,

Your obedient servant,

J. BOSTOCK.

XII.

XII.

Method of increasing the Durability of Tiles; by Count VON MELLIN*.

Methods of rendering tiles more durable.

MEANS of increasing the hardness of tiles, and consequently their durability, have been sought, without the discovering of any sufficiently cheap and simple for common use. Such is the glazing or varnishing of tiles, which is indeed very good, but too expensive, to be generally adopted.

Glazing expensive.

Coating with tar cheap and effectual.

Tarring tiles has been proposed; and this process appearing to me easily practicable and not expensive, I determined to make trial of it on one of my roofs, that required a great deal of repairing.

Method employed.

Providing some of the largest brushes I could get, I and an assistant set ourselves to coat the upper side of my tiles with tar liquefied over a gentle fire, and kept moderately hot. Four persons were employed to hand us the tiles, and, when tarred, to lay them in the sun to dry; which took three or four days, it being then the spring of the year. It is proper to say, that I had set apart the best tiles, or those which appeared most thoroughly baked; and that I exposed the others to the sun, that they might be warmed and receive the coat of tar more easily. After the process these appeared as if coated with a reddish brown varnish. Four hours were sufficient for the preparation of two thousand.

Another method.

Near my house was a tile kiln, which was just ready to draw. As soon as it was sufficiently cool, to allow the tiles to be handled, I had as many taken out, as left in the interior of the kiln sufficient room for a few people to coat them with tar. While two of these were tarring the tiles, three others were employed to give them, receive them when tarred, and lay them in a corner of the kiln, where the heat was reduced to that of a vapour bath. When the kiln was quite cold, the tiles were perfectly dried, but they had not such a shining coat as the former, because the great heat

* Sonnini's Biblioth. phys.-écon. Oct. 1808, p. 243.

had

had caused the tar to penetrate into their substance. Their pores were completely stopped, and they were rendered impenetrable to water, as I found by experience. The five persons I have mentioned tarred four thousand tiles in six hours. Both these experiments did not consume a barrel of tar.

The tiles not injured in 29 years.

The roof for which these tiles were used is open to the north, and exposed to all the violence of winds and storms. It was repaired in 1779, and not one of the tarred tiles is at all injured or decayed. They are covered with a very fine moss; and their surface is in as good condition, as if the tar had just been laid on. On the other hand, several of those which, as I said before, I had set apart, supposing they would resist the weather without any preparation, because they were thoroughly burned, are cracked, broken at the corners, or splintered on the surface.

Additions to the tar injurious.

Some persons say, that tarred tiles would be more durable, if they were powdered with iron filings and charcoal dust: but I conceive these substances would render the surface rough; and thus detain the water, while those coated with varnish would let it run off.

Other substances.

I am of opinion however, that a mixture of lime and tar would be more beneficial. I think too, that fats in general, whale oil, or the dregs of our oils, would be equally adapted to the purpose, and still cheaper.

ANNOTATION.

Calculation of the expense in this country.

Few people in London or its vicinity, where tiles are the common covering of houses, but must have experienced great inconvenience from roofs leaking, and the consequent trouble and expense of frequent repairs. Sometimes indeed this is owing to the badness of the mortar employed; but is most commonly the consequence of a few tiles being cracked to pieces by frost, after they had imbibed water. The method above recommended would appear to be a sufficient remedy for this; and the expense attending it is not an object at all comparable with the comfort and advantage of a secure roof. I am not certain, whether the count be

be speaking of plain tiles, or pantiles; but taking them to be plain tiles, the least favourable supposition, and the size of ours, a roof of 24 feet by 25, which would be that of a house of middling size would take about 4000. Now two thirds of a barrel of tar, at £2 6s. a barrel, the highest price in the market at the present time, come to £1 10s. 8d; and the labour, at the rate of 6 men for 8 hours, the longest time in the two experiments above, at 5s. a day, will be £1 4s; so that the whole additional cost of a moderate sized roof would not exceed £2 15s. This must very soon be reimbursed by the saving in repairs of the roof alone; and all the inconvenience beside the injury done to the ceilings and goods would be avoided. If coal tar were used, which, I should imagine would perfectly answer the purpose, supposing such a roof to require a hundred weight, this now sells for 18s, so that the cost would be only two guineas. C.

XIII.

Extract of a Letter from Prof. Kries, *of Gotha, to Mr.* Gehlen, *on Radiant Heat**.

I Imagine a short historical note on radiant heat will not be unpleasing to your readers.

Experiments on radiant heat in the 17th century.

The experiments of Pictet, made with two mirrors, in the focus of one of which he placed a burning body, and thus set fire to combustible substances in the focus of the other, justly excited the attention of natural philosophers. But it is surprising, that such experiments had been made more than a hundred years before, but were forgotten. Lambert, in his Pyrometry, says, on the authority of Zahn, that the experiment of collecting heat from a charcoal fire by a mirror of 18 inches diameter, and reflecting it to the distance of 20 or 24 feet to a smaller mirror of 9 inches, which so concentrated the rays, that tinder and matches were

* Ann. de Chim. vol. LXXI, p. 158. Translated from Gehlen's Journal by Tassaert.

kindled

kindled by them, had long ago been made at Vienna. The work of Zahn referred to is no doubt his *Oculus artificialis**, which was published in 1685.

I have a small tract entitled: Account of the parabolic wooden Mirrors, and their surprising action, which were lately invented by Andrew Gaertner, Machinist, and Model-Master to the King of Poland and Elector of Saxony, at Dresden, 1785.

Wooden mirrors of great power.

In this work is the following description of an experiment. "I placed a live coal before the mirror, in its focus, and immediately the mirror diffused a strong heat to the distance of forty or fifty paces, which it would not do with the sun. Hence I conceive, that what is told of the celebrated Archimedes is true: though that he did not produce so powerful a heat by the help of the solar rays, but by a fire kindled for the purpose; for when I placed a small mirror, half an ell [near a foot] in diameter, opposite the greater, and sixty paces from it, and the coal was made to burn bright, immediately the little mirror lighted a candle; which many persons would not believe, till they had seen it. I tried also whether a lighted candle would produce the same effect as the coal; but it did not, for there was not the least heat from it."

Experiments with them.

The large mirror here alluded to was 2½ feet in diameter: the largest of the author's making was five.

Size of the mirrors.

The following experiment appears to me still more surprising. The author says: "if I held the mirror ten or twelve paces from a hot iron stove, immediately it kindled a fire: and the same thing took place on holding it 20 or 24 paces from a fire in an open fire place."

Other experiments.

Pictet's experiment with ice which surprised him so much, is described in the same work.

A little after the experiments I have just mentioned, the author continues thus, "If, instead of fire, I placed cold water in the focus of the mirror, it diffused an agreeable coldness even in the height of summer: and if, instead of water, I used ice, very considerable cold was produced at the distance of ten or twenty paces.

Radiant cold.

* More probably, I should think, his mundi Œconomia, or his Specula physicoomathematicohistorica. C.

Dolf,

Structure of the mirrors

Dolf, who has spoken of Gaertner's gilt mirrors, and after him Gehlen, and lastly Fischer, all say, that it is not known how Gaertner made his mirrors. The following is the account he gives in his own work.

"All these mirrors are of common wood; more of soft wood than of hard. For, though I have made mirrors of all sorts with metals, yet what I have said is to be understood of wooden mirrors only, which must be gilt within and without. If any amateur wish to make such mirrors himself, he has only to apply to the inventor, who will inform him and instruct him in every step necessary to be taken both for making and gilding them."

Gaertner therefore, though he does not [illegible] fabricated them, made no mystery of it. It is [illegible] he directs the mirrors to be gilt on both sides. Perhaps this tended to preserve the mirror, and prevent its being warped by dryness: perhaps in this way he had a convex and a concave mirror in one. but he says nothing on the subject.

XIV.

Method of keeping Green Pease and French Beans.

Method of keeping green pease,

INTO a middling sized stewpan, filled with young green pease, put two or three tablespoonfuls of sugar, and place the pan on a brisk charcoal fire. As soon as the pease begin to feel the heat, stir them two or three times, and, when they yield water, pour them out on a dish, to drain off the water that comes from them. When drained, spread them out on paper in an airy room, where the sun does not come, and, that they may dry the more quickly, turn them frequently. It is necessary for their keeping, that they should not retain any moisture, if they do, they will soon grow mouldy.

and French beans.

French beans may be managed in the same way, and thus they will keep till the next season as well flavoured as when first gathered.

* Sonnini's Bibliothèque Physico économ. Aug 1808, p 105

XV.

XV.

On the Art of Printing from Stone. Communicated by a Correspondent.

IN your last number you inserted from the "Annales de Chimie" an account of the method of printing from stone. It is certainly an ingenious, and most probably a useful art; though I believe very little known in England. There are one or two circumstances respecting it, with which some of your readers may like to be acquainted, that Mr. de Serres [illegible]. A close texture, as he observes, is advantageous, and indeed necessary to its giving a clean impression.

Art of printing from stone little known here.

I made the ink according to his direction, (which was considered so great a secret) but prefer to it coloured turpentine, copal, or lac varnish. Muriatic acid is cheaper than nitric acid, and has the advantage of not acting upon the resin or wax, which forms the base of the varnish used.

Ink for it.

Muriatic acid preferable to nitric.

After purchasing some pieces of marble, I was much vexed to find that both the muriatic and nitric acid left some of the veins untouched, and only partially dissolved others; this must be attended to in selecting the blocks. I find some pieces of the limestone from Clifton near Bristol take a tolerable polish, and dissolve readily.

Choice of the stone.

But the easiest and cheapest way for those who wish to have a card, ciphers, &c., is Chauvron's on stone or even on lead. A little piece may be executed in a quarter of an hour; and if wetting is not sufficient to prevent the ink from adhering to the block, it will bear sponging, and yet leave enough of the ink upon the figures.

[illegible] method to small articles.

The insertion of these hints in your valuable Journal will oblige your constant reader.

G. O.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

AT a meeting of this Society, on the 1[illegible]th of May last, the Rev. John Fleming, of Bressay, read an account of several

Rare animals in Shetland.

several rare animals found by him in Shetland; particularly pleuronectes punctatus, a specimen of which he exhibited to the Society; lucernaria quadricornis; echinus miliaris, &c.; and some undescribed species, particularly a flustra, which he proposed to call *flustra Ellisii*, in honour of Mr. Ellis, the illustrator of the corallines. At the same meeting, Dr. John Barclay read remarks on some parts of the structure of the large marine animal cast ashore in Stronsay last year.

Germination of grasses.

At a meeting on the 26th of May, Dr. John Yule read a [illegible] of experiments and observations on the germination [illegible] graminea, in which he stated some [illegible] facts respecting the economy of this useful class of [illegible], illustrated by a series of drawings and specimens [illegible] germinated seeds of the cerealia or cultivated species, and of the buds of the stem, and panicle of viviparous grasses. And the Secretary read a communication from William Fitton, Esq., on the porcelain earth of Cornwall.

Copleyan Medal.

The Copleyan medal for last year was adjudged by the Royal Society to Mr. Edward Troughton, for the account of his method of dividing astronomical instruments, printed in the last volume of the Philos. Trans. See Journal, vol. XXV, p. 1 and 100.

Eye of the codfish.

Mr. Albers, of Bremen, having examined the eye of the codfish, gadus morrhua, L., finds, that the sclerotica is composed of two coats; the outermost of which is membranous, while the innermost is horny, and divisible into several layers. The innermost of these layers is coated with a substance resembling spermaceti, which forms little cysts, containing water in their intervals. This separates the sclerotica from the outermost silvery coat of the choroides, which by maceration in water becomes divisible into two layers. The intermediate coat of the three principal ones, that occupy the place of the choroides in the eyes of fishes, is composed in the cod of a retiform vascular texture, the trunks of which issue from the horseshoe shaped tubercle peculiar to fishes, and the use of which is so difficult to explain.

Some

Some anatomists have taken this tubercle for a gland, others for a muscle; but it appears, from the minutest research, to be a convolution of the larger bloodvessels, and Mr. Albers considers it as a sort of reservoir for the blood, a kind of *rete mirabile*. Tubercle in the eyes of fishes.

The air bladder of the sapphirine gurnard, trigla hirundo, L., differs very remarkably in its internal structure, as well as in its shape, from that of any other known fish. It is near three inches long, two broad, and more than one deep. At one end it has the appearance of having received a deep gash, and at the other of two. The first is formed by a slight separation of about three quarters of an inch; the other by two perpendicular separations, nearly parallel, and more than two inches long. Air bladder of the tubfish.

The loss of any one of the senses is certainly a serious calamity, but that of sight is on many accounts the most important. Every attempt to alleviate this loss therefore deserves encouragement, and may be expected to meet success proportionate to its merit. The establishment of Mr. Haüy at Paris, some remarks on which by Mr. Berard were given in our Journal, vol. III, p. 189, was formed near thirty years ago, and has been imitated we understand in other parts of Europe. The benefits, that have already resulted from the School for Indigent Blind in St. George's Fields, are no doubt known to many of our readers; but from a participation in these the children of the wealthy are excluded, nor indeed would they be adequate to their wants. We are happy therefore to learn, that an establishment is recently formed at Chelsea for teaching not only reading and writing, but the various sciences, to the opulent blind; so that the children of the rich, labouring under this misfortune, may have it alleviated as far as is practicable. The plan appears to be, on the whole, highly commendable: but, as it is yet in its infancy, the author, Mr. Bonner, would be happy to receive any suggestions, that could tend to its improvement. Establishment for the education of the blind.

To CORRESPONDENTS.

Mr. Shute's paper was too late for insertion this month.

Meteorological

METEOROLOGICAL JOURNAL

For JULY, 1810,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

JUNE Day of	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night	BAROMETER, 9 A. M.	WEATHER. Day.	Night.
27	58°	58°	63°	50°	30·01	Fair	Fair*
28	59	60·5	66·5	55	30·30	Ditto	Cloudy*
29	64·5	60	68	51	29·98	Rain	Ditto
30	60	64	70·5	56·5	30·13	Fair	Ditto
JULY							
1	54·5	66	74·5	59	30·11	Fair†	Rain
2	64	68	72·5	56	29·86	Ditto	Fair
3	64	56	65	51·5	29·67	Ditto	Ditto
4	58	56	61	51	29·39	Rain	Ditto
5	59	62	65·5	58	29·77	Ditto	Ditto
6	63	63	69·5	54	29·91	Fair	Ditto
7	63	67	71·5	61	30·03	Ditto	Ditto
8	67	61·5	68	55	29·80	Rain	Ditto
9	62	63·5	70	58	29·95	Fair	Ditto
10	64	63·5	68	59·5	29·76	Ditto	Ditto
11	65	65	73·5	53	29·64	Ditto	Ditto
12	64	64	68	58	29·56	Ditto	Cloudy‡
13	64·5	64	70	55	29·61	Rain	Fair
14	63	55	65	48·5	29·71	Ditto§	Ditto
15	55·5	56	66	48	29·95	Ditto	Ditto
16	59	57	62	50·5	30·03	Ditto	Rain
17	55	58	62·5	51	29·82	Cloudy	Cloudy
18	56	58	64	51·5	29·58	Fair	Fair
19	57·5	60·5	65	51·5	29·84	Ditto	Ditto
20	59	58·5	65	51	29·79	Ditto	Ditto
21	55	54	60·5	48·5	29·92	Ditto	Ditto
22	56	59	63	50	30·10	Ditto	Ditto
23	59	60·5	70	54	30·17	Ditto	Ditto
24	61	61·5	70	53·5	30·17	Ditto	Ditto
25	63	61·5	71·5	59·5	29·88	Rain	Cloudy
26	66	56	66·5	53	29·80	Ditto	Ditto

* Thunder, lightning and heavy rain in the night.

† Storm commencing about 3 P. M. Boisterous at 8, with thunder and lightning, continuing most of the night, with rain.

‡ Rain at half past 11 o'clock.

§ At 2 P. M. storm of hail, rain, thunder, and lightning, the thermometer retiring to 55. Some hail stones near half an inch diameter.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SUPPLEMENT TO VOL. XXVI.

ARTICLE I.

The Bakerian Lecture for 1809. *On some new Electro-chemical Researches, on various Objects, particularly the metallic Bodies from the Alkalis, and Earths, and on some Combinations of Hidrogen, By* HUMPHRY DAVY, *Esq. Sec. R. S. F. R. S. E. M. I. R. A.**

1. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries, on the application of electricity to chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

Application of electricity to chemistry pursued.

In this communication, I shall, as formerly, state the results. I hope they will be found to lead to some views and applications, not unconnected with the object of the Bakerian lecture: and though many of them are far from having attained that precision and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

* Philos. Trans. for 1810, p. 16.

Explanation of the Figures.

Explanation of the plates.

Pl. IX, Fig. 1. The apparatus for electrizing potassium in gasses. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water out of the contact of air. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. a, d, c, the level of the different fluids.

Pl. X, Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £1000 has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected, from so great an accumulation of electrical power.

II. *Some new Experiments on the Metals from the fixed Alkalis.*

Metals of the fixed alkalis.

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Different hypothetical explanations of the facts.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists

chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

Messrs. Gay-Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hidrogen; a similar opinion seems to be entertained by Mr. Ritter. Mr. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hidrogen, with the alkalis; and an inquirer † in our own country regards them as composed of *oxigen* and *hidrogen*.

I shall examine such of these notions only as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

Gay-Lussac and Thenard's founded on the action of potassium on ammonia.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which Messrs. Gay-Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mém. d'Arcueil, a copy of which Mr. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail it seems, that they still retain their opinion; but upon precisely the same grounds as those, to which I have before referred. That no step of the discussion may be lost to the Society, I shall venture to state fully their method of operation, and of reasoning.

* Journal de Physique, June, 1808; or Journal, vol. xxiv, p. 40.

† Nicholson's Journal, August, 1809, p. 258

Their method of operation, They say, that they heated potassium * in ammonia, and they found, that a considerable quantity of ammonia was absorbed, and hidrogen produced; and that the potassium became converted into an olive coloured fusible substance. By heating this substance strongly, they obtained three fifths of the ammonia again, two fifths as ammonia, one fifth as hidrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two fifths, and found in the vessel, in which the operation was carried on, nothing but potash.—Again, it is stated, that, by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hidrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia more than its volume of hidrogen.

and reasoning on it. Whence, they ask, can the hidrogen proceed?—shall it be admitted, that it is from the ammonia? but this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water, which may be supposed to be in the ammonia, or from the metal itself. But the experiments of Mr. Berthollet, jun., prove, that ammonia does not contain any sensible quantity of water. Therefore, say they, the hidrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hidrogen, and that alkali.

Defect in their statement. It is obvious, that, even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm, that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxigen which this would give to the potassium, and of the hidrogen which it might furnish, to reproduce the ammonia, they take no notice.

Mr Davy's results very different, I have shown, by numerous experiments, many of which have been repeated before members of this Society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French

* Mém. d'Arcueil, Tom. II, p. 309.

chemists,

chemists, when the operations are conducted in a refined and accurate manner.

from cautious proceeding.

Little ammonia regenerated,

In proportion as more precautions are taken to prevent moisture from being communicated to it, so, in proportion, is less ammonia regenerated; and I have seldom obtained as much as [illegible] of the quantity absorbed. And I have never procured hidrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

and an excess of nitrogen produced

Potassium always revived.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show, that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed page 458 of the last volume of the Transactions*, and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

Potassium heated in ammonia

I employed a tube of platina bored from a single piece, which, having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort: the potassium was employed in quantities of from 3 to 4 grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

rapidly.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation; and in these cases, there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment. But when the heat was very slowly raised, the loss was much less considerable, and in several cases, I obtained more than four fifths of the potassium, which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

and very slowly.

Experiment detailed.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30·2 in.; thermometer at 54° Fahrenheit. Three grains and a

* Journal, vol. xxv, p. 143.

half

half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hidrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hidrogen. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied, by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxigen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed was such as to generate, by its action upon water, 3 cubical inches and $\frac{3}{10}$ of hidrogen gas.

Results.

Now if this experiment be calculated upon, it will be found that 7·5 − ·2 = to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hidrogen. But the 3·2 cubical inches of hidrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process = 9; and the nitrogen in the 8·8 cubical inches of gas, (or the 9 − ·2 of ammonia,) will be about 3; and if we estimate ·34 of hidrogen, and ·16 of nitrogen, in the 5 remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{8}{10}$ of hidrogen preexisting in the tube and adaptors, the loss of hidrogen will be found proportionably rather greater than that of nitrogen.

Another experiment.

In another experiment, in which 3 grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: [illegible] cubical inches of gas were produced; and there must have remained in the tubes and adaptors the same quantity of residual air, as in the process last described.

The

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxigen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2 in., thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them. Results.

Now 6·78—·4 of ammonia = 6·38, and this quantity of gas, decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hidrogen. But there are produced in this experiment, of hidrogen, 2·48 in the first operation, and 4·28 in the second; and, considering the nitrogen in the permanent gas as 3·32, 0·8 must be subtracted from the common air; which would give 2·52 for the nitrogen generated: and to these must be added the quantity of hidrogen and nitrogen in the tubes and adaptors. Deductions from them.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hidrogen.

In all experiments of this kind a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water. Black matter separated.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina. I exposed some of it to heat in a retort, containing oxigen gas; there was a diminution of the gas, and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water. This examined.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found, that in the process in which from 3 to 4 grains of potassium were made to act upon ammonia in a vessel of platina, and afterward distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no farther in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxigen. A compound of platina (and hidrogen?)

In

In iron tubes, nitrogen lost, and hidrogen produced;

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hidrogen.

in copper, less hidrogen;

When copper tubes are employed, the hidrogen bears a smaller proportion to the nitrogen; and more potassium is revived.

in platina, hidrogen lost.

In those experiments, in which platina has been used, there is little or no loss of potassium or nitrogen: but a loss, greater or smaller, of hidrogen.

On what do these results depend?

It will be asked, on what do these circumstances depend? Do the affinity of certain metals for potassium prevent it from gaining oxigen from ammonia, and do platina and copper combine with a small quantity of hidrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia it is not a *metallic* body that is decomposed, but the volatile alkali; and that the *hidrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxigen and hidrogen.

Action of sodium on ammonia.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3·3 grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hidrogen; and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced equalled twelve cubical inches; and,

and, by detonation with oxigen, proved to consist of nearly two of hidrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention the mere visible phenomena of the action of sodium on ammonia cannot, I conceive, fail to be convinced, that it is the volatile alkali, and not the metal, which is decomposed in this process.

The volatile alkali decomposed.

As sodium does not act so violently upon oxigen, as potassium, and as soda does not absorb water from the atmosphere with nearly so much rapidity as potash, sodium can be introduced into ammonia much freer from moisture than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green; it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form evidently by combining with one part of the elementary matter of ammonia, while another part is suffered to escape in the form of hidrogen.

Phenomena of the action of sodium on ammonia.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of Mr. Curaudau, that the metals of the alkalis are composed of the *alkalis* merely united to *charcoal*. The investigation upon which he has founded his conclusions is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which Messrs. Gay-Lussac and Thenard have been misled by the moisture, which interfered with their results.

Mr. Curaudau's hypothesis

Mr. Curaudau states, that, when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxigen: nearly two cubical inches of oxigen were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium

refuted.

 were

were made to act upon water; they decomposed it with the phenomena, which I have described in the Bakerian lecture for 1807 *. Nearly 6 cubical inches of hidrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalis, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from Mr. Curaudau's method of decomposing the alkalis, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

Mr. Ritter's hypothesis

Mr. Ritter's argument in favour of potassium and sodium being compounds of hidrogen is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one

refuted.

is more easily answered. Sodium absorbs much more oxigen than potassium, and, on the hypothesis of hidrogenation, must contain much more hidrogen; yet, though soda is said to be lighter than potash in the proportion of 13 to 17 nearly †, sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxigen than sodium, and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

His observations on the action of tellurium upon water.

Mr. Ritter has stated, that, of all the metallic substances he tried for producing potassium by negative voltaic electricity, tellurium was the only one, by which he could not procure it. And he states the very curious fact, that, when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxigen is given off at the positive surface; no hidrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives, that the reason why tellurium prevents the metallization of potash is,

* Journal, vol. xx, p. 307.

† Hassenfratz, Annal. de Chem, tom. xxviii, p. 11.

that

that it has a stronger attraction for hidrogen, than that alkali *.

This does not prove potassium to be a compound.

These circumstances of the action of tellurium upon water are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some expe. periments on the subject, and on the action of tellurium on potassium, and I find, that, instead of proving that potassium is a compound of potash and hidrogen, they confirm theidea of its being as yet, like other metals, undecomposed.

Experiments on it, the tellurium being positively and negatively electrified,

When tellurium is made the positive surface in water, oxigen is given off. When it is made the negative surface, the voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hidrogen in water; which, in being diffused, is acted upon by the oxigen of the common air, dissolved in the water, and gradually loses a part of its hidrogen, and becomes a solid hydruret of tellurium. The compound of hidrogen and tellurium produced at the negative pole, when uncombined, is gaseous at common temparatures; and when muriatic acid, or sulphuric acid, is present in the water, it is not dissolved, but is given off, and may be collected and examined.

A compound of tellurium and hidrogen produced.

Experiment with tellurium negatively electrified.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found at the beginning of this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which, when touched by water, did not inflame or effervesce, but rendered the water of a beautiful purple colour, and when thrown into

* Journal, vol. xxiv, p. 318.

water

water entirely dissolved, making a bright purple tincture.

A compound of tellurium, hidrogen, oxigen, and potassium, produced.

It immediately occurred to me, that the whole of the hidrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium; and that the *telluretted* hidrogen, (if the name may be used,) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water: and this I found to be the case; for, on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hidrogen; metallic tellurium was formed where it came into contact with the air, and muriate of potash was found dissolved in the mixture.

Action of potassium on tellurium.

It seemed evident from this fact, that, in the action of tellurium negatively electrified upon potash, potassium was produced, as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was farther confirmed by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hidrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hidrogen was evolved by the action of the alloy upon water; but the compound of telluretted hidrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

Oxides of potassium and tellurium reved together by charcoal.

The very intense affinity of potassium and tellurium for each other induced me to conceive, that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time by heated charcoal; and I soon proved, that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off,

off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

Accidental alloy of tellurium with potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found, that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

Aeriform compound of tellurium and hidrogen.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hidrogen; I shall mention merely some of its most remarkable qualities and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hidrogen is more analogous to sulphuretted hidrogen, than to any other body. The smell of the two substances is almost precisely the same*. Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened

Supposition, that tellurium might contain sulphur.

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children Esq., of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton cooperated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive, that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of voltaic electricity, or of potassium; and some researches made upon the habitudes of different metallic sulphurets, at the voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hidrogen in the voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxigenation, could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and farther researches, made in the laboratory of the Royal Institution, proved, that the substance in question was a new and singular combination.

litmus

litmus; but, after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalis. It precipitates most metallic solutions. It is instantly decomposed by oximuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium*.

Arsenic

As arsenic has an affinity for hidrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

made the negative surface in water,

Arsenic made the negative surface in water, by means of a part of the new battery containing 600 double plates, became dark coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

in a solution of potash,

Arsenic negatively electrified in a solution of potash likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid.

and in contact, with solid potash.

When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hidrogen by the action of water with inflammation, and deposited a brown powder.

Potassium and

When potassium and arsenic † were heated together in hidrogen

* From the results of one experiment, which I tried, it seems that tellurium, merely by being heated strongly in dry hidrogen, enters into combination with it. An accident prevented me from ascertaining, whether the compound so formed is exactly the same as that described in the text.

Volatile pyrophorus from

† In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash

drogen gas, they combined with such violence, as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the voltaic battery. arsenic heated in hidrogen gas.

As tellurium and arsenic both combine with hidrogen, it appeared to me probable, that, by the action of alloys of potassium with tellurium and arsenic upon ammonia, some new phenomena would be obtained, and probably still farther proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four sixths nitrogen, instead of being pure hidrogen, as in the case of the action of potassium alone. Action of alloy of tellurium and potassium on ammonia.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas, which was principally nitrogen, so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hidrogen, which are contradictory assumptions. Action of alloy of arsenic and potassium on ammonia.

None of the chemists, who have speculated upon the *imaginary hidrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon Hidrogenation of potash not proved by analysis or synthesis.

potash and white oxide of arsenic, Fourcroy, Connais. Chem. Tom. viii, p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that, though potash is decomposed in this operation, yet the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hidrogen. The gasses not absorbable by water, given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic, and hidrogen: whether they are mixtures of various gasses, or a single compound, I am not at present able to decide. oxide of arsenic and acetate of potash.

distant

distant analogies, or upon experiments in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to show, that, when potassium or sodium is burnt in oxigen gas, water is formed; or that water is generated, when potassium decomposes any of the acids*: and no one has been able to form potassium, by combining hidrogen with potash. I stated in the Bakerian lecture for 1807, that, when potassium and sodium were burnt in oxigen gas, *the pure* alkalis were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxigen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find, that this is the case; for the results differ very little in some experiments, which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

Oxigen consumed proportional to the quantity of metal.

When potassium is burnt in trays of platina, in oxigen gas that has been dried by ignited potash, the absorption of oxigen is about $\frac{11}{10}$ of a cubical inch for every grain of the

Decomposition of boracic acid.

* When, in October 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the voltaic circuit, I concluded, that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808 I made farther experiments on this substance, and ascertained, that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance, as I had procured by electricity. Messrs Gay-Lussac and Thenard have likewise operated upon boracic acid by potassium, and they conclude, that they have decomposed it; but this does not follow from their theory, unless they prove, that water is given off in the operation, or combined with the borate of potash; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made hydruret of boracic acid.

metal

metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain *. The alkalis so formed are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalis, give indications of the presence of moisture.

Mr. d'Arcet has shown by some very well conducted inquiries, that potash and soda †, in their common state, contain a considerable proportion of water; and Mr. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in 100 parts 66·66 potash and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

Potash and soda contain much water.

Potash loses its water when combining with muriatic acid.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hidrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances I obtained from 10 parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash in muriatic acid gas. I shall give the results of two experiments made in this manner: 5 grains of potassium, inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire,

Potassium converted into potash upon the surface of muriatic acid,

and in muriatic acid gas. Exp. 1.

* The quantities of gas given out by the operation of water are in a similar ratio. See page 43 of the last Bakerian lecture [Journal vol. xxiii, p. 245], and page 330 of this paper.

† Annales de Chimie. Nov. 1808, page 175; or Journal for September.

burnt with a beautiful red light *, and the whole mass appeared in igneous fusion; a little muriate of potash, in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hidrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

Exp. 2. The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium, where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Deductions. Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxigen, to form 9·4 grains of potash, and 6·6—1·4 = 5·2, the quantity of muriatic acid combined with the potash; which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash. But 35·6 of muriatic acid, according to Mr. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash; and 71·1—64·4 = 6·7; so that the potash taken as a standard by Mr. Berthollet contains at least 9 per cent more water, than

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

 that

that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali *.

After these illustrations, I trust the former opinions, which I ventured to bring forward, concerning the metals of the fixed alkalis, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist. Potash and soda pure metallic oxides.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hidrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalis, and oxides. This part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

(To be continued.)

II.

Of the Spinellane: and some other fossil Substances, by Mr. Nose †.

Mr. Nose has described this substance in his mineralogy of the mountains of the Rhine. He found it on the banks of the lake of Laach, near Andernach. It is in a rock composed of various substances, as oxide of iron, quartz, hornblende, mica, and some other substances, to which he has given peculiar names, such as, Spinellane where found.

* Consequently Mr. Berthollet's fused potash must contain nearly 23 per cent of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent of water, taking the potash formed by the combustion of potassium as the dry standard. Water in potash.

† Journal de Physique, vol. lxix, p. 160.

Sanidin. 1, A variety of tabular feldspar, which he calls *sanidin*;

Desmin. 2, Another substance, crystallized in small silky tufts, which he calls *desmin*.

Characters of the spinellane. The spinellane has a brownish colour.

Its form appears to be that of a hexaedral prism, terminated by triedral pyramids with rhomboidal faces.

Mr. Nose however imagines, that he has observed a great many points of resemblance between it and the balass ruby, or spinelle, whence he has been led to give it the name of spinellane.

It does not rank very high in point of hardness, but is sufficiently hard to scratch glass.

III.

Researches on Acetic Acid, and some Acetates: By RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A. &c.*

(Concluded from p. 237.)

Saturating power of the solution of potash ascertained.

TO ascertain the saturating power of this solution of potash however, I neutralized with it muriatic acid diluted so as to be of the specific gravity of 1·0707. Of this 10·000 parts required 27·448 of solution of potash. I then precipitated an equal quantity of the same acid by nitrate of silver, and I obtained 5·050 of muriate of silver, which gives us a fixed standard for the solution of potash, and renders the experiments capable of being compared together, and with others. The specific gravity of the solution of potash was 1·0786.

Separation of the spirituous liquor by carbonate of potash.

Carbonate of potash thrown into a fluid containing a spirituous liquor is divided into two portions: but if the fluid contain acetic acid also, or, I believe, any other acid, that forms with the potash a salt soluble in spirit, the separation does not take place; particularly if the acid contained in it be much more in quantity than the spirituous liquor. For this reason I have been forced to distil the fluid products of acetates of silver, copper, and nickel, from potash, till all their acid was extracted, and examine them afterward for the spirituous liquor.

I in-

I introduced a hundred parts, by measure, of the liquid to be assayed into a tube, which was 5 dec. [19·6 inch.] long, and 5 or 6 mil. [0·2 of an inch] in diameter, and closed each end by a small cork. I put in carbonate of potash sufficient to render the separation complete, and in order to bring the spirituous liquor to the part of the tube, where I had measured the whole of the liquid before I put in the carbonate of potash, I let out a portion of the liquid below, by drawing out the cork from the bottom, so as to compensate the increase of bulk occasioned by the addition of carbonate of potash. Thus I avoided the errours, that any inequality in the bore of the tube would have occasioned.

The pyroacetic spirit thus separated is not at its greatest degree of dryness: but it is in a sensibly uniform state, and may be subsequently rectified by other processes.

In a similar way I proceeded with all the liquid mentioned in this paper.

As to the aeriform products, the separation of the carbonic acid was effected in the solution of barytes contained in the first phial. I ascertained its quantity by examining the carbonate of barytes formed during the process. The hidrogen gas I collected in the pneumatic apparatus. Aeriform products.

I will not assert, that there is exactly the same precision with regard to all the acetates; but the variations appeared to me too small, to be noticed in researches of this kind. If an acetate of the same metal be distilled several times, we shall find slight variations from one experiment to another, which require numerous repetitions, that we may take the mean of as many operations as possible. We should also take every care, to preserve the same degree of temperature throughout every operation, as well as in all that are undertaken with a view of comparing the different acetates with each other. Calculations.

These variations take place most generally in the acetates that yield the greatest quantity of pyroacetic spirit; and it was by taking the mean terms of several operations, and particularly attending to the greatest and smallest quantities of each substance produced in distilling all these salts, that I determined the proportions and order in the following general table.

Tabulated results.*

		Acetate of silver.	Acetate of nickel.	Acetate of copper.	Acetate of lead.	Acetate of iron.	Acetate of zinc.	Acetate of manganese
	Loss in the fire.	0·36	0·61	0·64	0·37	0·49		0·555
Residuum in the retort.	State of the base.	Metallic.	Metallic.	Metallic.	Metallic.	Black oxide.	White oxide.	Brown oxide.
	Residual carbon.	0·05	0·14	0·055	0·04	0·02	0·05	0 035
Liquid products	Specific gravity.	1·0656	1·0398	1·0556	0·9407	1·011	0·8452	0 8364
	Proportion of acidity.	107·309	·44·731	84·868	3·045	27·236	2 258	1·285
	Spirituous liquor.	0	2	0·17	0 555	0·24	0·695	0 94
Aeriform products.	Carbonic acid.	8	35	10	20	18	16	20
	Carburetted hidrogen.	12	60	34	8	·34	28	32
	Total of gas.	20	95	41	28	52	41	52

In this table it may be remarked, that the specific gravity of the product of the distillation of acetate of silver is but 1·0656, while its degree of acidity greatly exceeds that of the rest: yet it does not contain any sensible portion of pyro-acetic spirit. At first I suspected, that this product might contain some other vegetable acid, beside the acetic: and I had not to choose among a great number, for few would have resisted the heat this product had undergone without being decomposed or volatilized. I saturated a portion with potash, and sought in vain for some other acid. I chiefly expected to find in it pyrotartaric acid, but it did not form the least precipitate with acetate of lead. Anomaly in the acetate of silver.

This fact may be explained by the tendency the concentrated acetic acid has to become solid, and the expansion it would undergo a little before the instant of congelation, analogous to what sir Charles Blagden observed in water. I exposed to the same temperature the products of the distilled acetates of silver, nickel, and copper, and that from silver crystallized first. It was likewise the last liquefied on raising the temperature anew; which tends slightly to support the explanation I have given.

With 15 gram. [232 grs.] of the liquid product of the distillation of the acetate of silver I mingled water by a gramme at a time. The specific gravity went on increasing, till three grammes of water had been added, when it was 1·0733; and the degree of its acidity, according to the component parts, 76·8959. With five grammes of water the spec. grav. was 1·0693. From five grammes to ten there appeared to me some slight variation; but beyond that proportion, when it was 1·0597, it decreased uniformly. I had no opportunity of examining this series of mixtures but once, but it appears to favour my opinion. It must be confessed however, that the effect is very great to be produced by so slight a cause. Specific gravity of different compounds of the acid from it with water.

This part of my researches however I have not examined sufficiently to decide. It is to be wished, that the exact ratios between the specific gravity and acidity of a liquor containing Mixtures of acetic acid and water should be examined.

containing acetic acid, without any other substance, should be ascertained*.

Crystallization of acetic acid not owing to spirit.

What I have just said of the congelation of the liquid product of the distillation of acetate of silver proves, that the crystallization of acetic acid is no way owing to the presence of the spirituous liquor. In fact how can we imagine, that a fluid congealable only at a very great degree of cold can increase the crystallizableness of another fluid, which crystallizes much more readily than itself? No cause for the crystallization of tartaric, oxalic, or any other acid has ever been sought for, but its peculiar nature.

Purest and strongest from acetate of silver.

The purest and most concentrated acetic acid I ever saw was that I obtained from the acetate of silver by distillation.

Acetate of nickel.

There seems to be some anomaly in the products of the distillation of acetate of nickel. The quantity of carbon in the residuum in the retort is very great, and there is likewise a great deal carried off by the gas. The difficulty of obtaining a sufficient quantity of this metal prevented me from carrying my examination farther.

Little spirit from the acetates of silver, nickel, and copper,

I cannot answer to a few hundredths for the quantity of spirituous liquor contained in the products of the distillation of the acetates of silver, nickel, and copper, on account of the small quantity they contain, and the larger proportion of their acetic acid. The acetate of silver appears to me to contain none. I am not so certain with regard to that of nickel. After having deprived the liquid product of the acetate of copper of a large portion of its acid by distilling it with carbonate of potash, I separated about 0 17 of spirituous liquor by the method used for the other liquid products.

and iron.

The acetate of iron is one of those most easily decomposed by heat. Accordingly it yields less of the spirituous liquor, and more acid, in the product of its distillation, than we have reason to expect from its other properties.

More in proportion as the metal less easily reducible.

Without arrogating to these results more confidence, than the nature of the research allows them to claim, we see, that from the four metals reduced to the metallic state during

* See the paper by Mr. Mollerat, Ann. de Chim., vol. lxviii, p. 88; or Journal, vol. xxv, p. 155.

the

the operation the quantity of pyroacetic spirit is uniformly greater in proportion as the metal is less easily reducible. Zinc, which does not alter its state, gives still more: and in this case the heat is necessarily greater, before the acid quits its base. Lastly manganese, which is not in the same state before and after the process, yields more of the spirituous liquor than zinc, because the base is capable of absorbing about 0·46 of oxigen, and with this the acid furnishes it.

Some preliminary experiments, by which I found that acetic acid contains a larger proportion of oxigen than the pyroacetic spirit, prove, that this acid furnishes it for the superoxidation of the oxide of manganese; and that by this mean an effect is produced, which in other circumstances takes place without it.

Has less oxigen than acetic acid.

This will be farther demonstrated by the distillation of earthy and alkaline acetates. I distilled the acetates of potash and soda, and obtained a more spirituous and less acid liquor, than any of those furnished by the metallic acetates. I obtained a similar result by the distillation of acetate of lime; but very pure and dry acetate of barytes, prepared with acetic acid made by Mr. Mollerat*, yielded me a liquid of the spec. grav. of 0·8458, which did not redden blue vegetable colours, and which, being treated with carbonate of potash to separate the pyroacetic spirit, did not allow the separation of a single drop. So far from this, having mixed a hundred measures of it with a hundred measures of water, and having afterward added carbonate of potash in the usual way, more than a hundred measures of pyroacetic spirit separated. The whole of the liquid therefore, arising from the distillation of acetate of barytes, was pyroacetic spirit more free from water than that from which carbonate of potash has taken all the water it can. Its specific gravity was greater than that of pure pyroacetic spirit, because it contained a yellow empyreumatic oil.

Liquid from the alkaline acetates more spirituous and less acid than from the metallic.

From acetate of barytes pure spirit alone.

These results prove, that, *cæteris paribus*, the production of pyroacetic spirit follows pretty exactly the inverse ratio

In the inverse ratio of the facility of decomposition.

* Consequently obtained from the distillation of wood. C.

ratio of the facility of decomposition by a high temperature: and that, if the acetate of manganese be an exception to this law, it is because its base facilitates the decomposition of the acid by absorbing oxigen.

No doubt there are other circumstances, that influence the results of these distillations; but the determination of such complicated causes is a problem not to be solved in the present state of chemistry.

Uniform in its properties.

Pyroacetic spirit is precisely the same substance, whatever be the salt that has served for its formation.

Products of acetates examined for prussic acid,

Prussic acid and ammonia have been classed among the products of the distillation of acetates. I have digested the liquid product of the acetates of lead and of potash on the red oxide of mercury, and on the black oxide of iron with potash. I have treated it with sulphate of iron, and by all the means I could think of; but I could not find any prussic acid, any more than in the dry residuum in the retort. Though it is extremely difficult, to detect very small quantities of prussic acid in any substance whatever, I do not think any exists in the liquid products of distilled acetates.

and for ammonia.

Visible vapours from muriatic acid no certain test of it.

As to the ammonia I added lime and potash to the liquid products of several acetates, and I afterward held near them a tube wetted with muriatic acid, when very visible vapours were produced. I could not distinguish the ammonia by the smell. I held the same tube moistened with muriatic acid near the surface of a little common alcohol, and observed the same vapours. I precipitated by a solution of potash a solution of acetate of copper, and added a little of the liquid product of distilled acetate of potash. Dividing it into two portions, into one I put a single drop of ammonia, and none into the other. I filtered the two separately, and then passed a stream of sulphuretted hidrogen gas through each. In that containing the drop of ammonia, brown flocks were formed, like hidrosulphuret of copper; but in the other I could distinguish nothing. From these experiments I am persuaded, that no ammonia is formed in the distillation of acetates; and that the mistake has arisen from the vapours, which muriatic acid forms

forms with the spirituous and volatile part of their respective products.

It is of little importance to know whether prussic acid or ammonia be contained in the products of acetates formed by an impure acetic acid like vinegar; for the extractive or mucilaginous matter might furnish either, without the acetic acid contributing to it. But as this acid is frequently the last result of the destructive distillation of vegetable substances, it is essential to know, whether it contain nitrogen.

Found perhaps when the acid was impure.

In all the products of the distillation of acetates, but particularly those that yield most pyroacetic spirit, we find a brown, fetid oil; but I have taken no notice of this in the table.

Brown fetid oil from acetates.

I endeavoured to modify the products of the distillation of the acetate of lead, by mixing with it other substances, either oxidating, or disoxidating. The acetate of lead mingled with a fourth of charcoal yielded 42.5 of pyroacetic spirit by distillation: and the same salt mingled with a fourth of black oxide of manganese yielded but 34. Their specific gravities were 0·9606 and 0·9633; and their ratio of acidity 2·445 and 2·052.

Acetate of lead distilled with charcoal, and with oxide of manganese.

The acetate of lead described by Thenard yielded 59 of pyroacetic spirit; 3·973 of acidity; and the spec. grav. of the whole liquid product was 0·9302.

It is certain, that heat alone is not sufficient, to convert acetic acid into pyroacetic spirit; but that the concurrence of other circumstances is necessary, which we are not yet able to ascertain. Into a porcelain tube, at a strong red heat, I passed acetic acid of the spec. grav. 1·0635, and the acidity of which was 60·624 The porcelain tube was luted to a tubulated matrass, and in the tubulure were placed two tubes. One of these, being open at both ends, allowed me to drain out the liquor, that condensed in the matrass, by means of a little pipe. The other, which was curved, reached to the bottom of a phial containing a solution of barytes, and communicating with a pneumatochemical trough. I continued the process eight hours, constantly returning into the retort the liquid that condensed in the matrass; so that ultimately each particle had passed five or

Heat alone does not convert pyroacetic acid into pyroacetic spirit.

six

six times through a tube heated red hot for the space of six inches. Carbonic acid and carburetted hidrogen gas were continually evolved. The liquid that passed into the matrass became more and more brown, and a small quantity of coally matter was left in the retort. The weight of the acid was then 1·0443, and the proportion of its acidity was only 33·65. No pyroacetic spirit was formed. Acetic acid therefore is capable of supporting a great heat, without being totally decomposed, and is at the same time highly volatile. For this reason it is almost always a result of the destructive distillation of vegetable and animal substances.

Heat diminishes its acidity more than its gravity.

It is to be observed that the acidity of the acetic acid, that has passed through a red hot tube, diminishes much more than its specific gravity. This is analogous to what happens in the result of distilled acetate of nickel. The cause I know not. I could not find any oil or other substance in these liquids; if they contain any other acid than the acetic, it must differ from this in a very few properties, for all those I examined agree perfectly with those of acetic acid.

Vapour of acetic acid passed through charcoal.

If carbon be introduced into the red hot porcelain tube before the vapour of acetic acid is passed through it, nothing is collected, even after a single distillation, but water, carbonic acid, and carburetted hidrogen gas.

The spirit obtained from acetic acid alone;

The distillation of some other salts formed by vegetable acids leads me to believe, that the acetic is the only acid, the salts of which furnish pyroacetic spirit by distillation.

not from tartaric,

I have distilled tartrate of copper and lead, and acid tartrate of potash. I have treated the products in the same manner as I did those of the acetates, but in no instance did I perceive any traces of the pyroacetic spirit.

oxalic,

The acid oxalate of potash also afforded me none. On comparing all the products of the distillation of the tartrate and oxalate of potash, I believe, that the tartaric acid differs from the oxalic chiefly by containing a larger proportion of carbon.

or citric.

I could not obtain any pyroacetic spirit from the distillation of citrate of potash.

As

As the pyroacetic spirit appears to be a constant and uniform substance, whatever may be the acetate from which it is derived, I have compared it with other spirituous liquors, in which the acetic acid might have had some influence. Compared with other spirits.

It is pretty generally imagined, that what is contained in vinegar is the result of the vinous fermentation, which during the distillation becomes ether by the action of the acetic acid. Mr. Gehlen very lately denied the direct formation of an acetic ether: but Mr. Thenard has since formed it, as other chemists had done before*. In March 1803 I obtained the following results, and I have since verified them. Formation of acetic ether.

I mixed together ten parts of alcohol, of the specific gravity of 0·8483, and ten of acetic acid exempt from all spirituous liquor, the spec. grav. of which was 1·0705, and 10 parts of which were neutralized by a quantity of base, which I shall represent by 49·587. The specific gravity of the mixture was 0·9150, though by calculation it should have been 0·9594. There was a very slight evolution of caloric, but I observed no other action, even at the expiration of eight and forty hours. I afterward distilled this mixture to dryness. The product had lost nothing perceptible of its weight, but its spec. grav. had become 0·9372. I distilled this liquid to dryness eight times over in close vessels; but its specific gravity did not alter after the first distillation. I neutralized 10 parts of this ether, which from its composition was equal to 5 parts of the acetic acid employed; and it took but 14·274 of base, instead of 24·793, the half of 49·587. Thus on distilling together 10 parts each of such alcohol and acetic acid as are described above, $\frac{10519}{24793}$, or nearly $\frac{5}{12}$, of the acid were expended in the etherification. Experiment.

To procure this ether free from acid, I saturated it with dry carbonate of potash; and added enough of this salt to take up all the water. Thus I obtained 7·40 parts of ether of the specific gravity of 0·8621. By adding a little of this to highly concentrated acetic acid, we have in a small The ether freed from acid. Concentrated vinegar.

* For its spontaneous formation see p. 157.

but

bulk a fluid, which makes a very pleasant vinegar by mixing it with water.

Pyroacetic spirit procured in quantity from acetate of lead.

To procure a sufficient quantity of pyroacetic spirit for comparing it with real acetic ether, I distilled ten pounds of acetate of lead. The apparatus consisted of an earthen retort, to which was luted a tubulated matrass, furnished with a tube reaching to the bottom of a very long proof phial, which was surrounded with a mixture of ice and muriate of soda. A second tube, straight and open at each end, allowed me to draw off the various liquid products collected in the matrass during the process. These I separated into two nearly equal portions, about 18 or 20 ounces each. A great deal of gas was evolved, which had a strong smell; but from which, when it came into the cold proof phial, was condensed a perfectly clear and colourless liquid, weighing about 3 ounces. I redistilled these products, dividing the first two again.

The following are the results.

Tabulated results.

		Specific gravity.	Proportion of acidity.	Ethereous liquor.
1st product	1st portion	0·9971	0·290	0·250
	2d portion	1·0063	0·525	scarcely perceptible.
2d product	1st portion	0·8177	scarcely perceptible.	9·625
	2d portion	0·8168	0·005	8·525
	3d portion	0·9972	1·353	1·475
3d product		0·7919	none	10·000

This operation afforded me a sufficient quantity of pyroacetic spirit, to be able to examine its properties.

Properties of the spirit.

It is perfectly colourless and limpid. Its taste is at first acid and burning; but afterward becomes cool, and in some sort urinous. It has the generic smell of volatile oils, without our being able to say of which in particular. Perhaps it comes near that of peppermint mingled with bitter almonds. The specific gravity of that condensed by cold at the extremity of the apparatus was, as we have seen, 0·7929 *.

* This, or the number in the table, must be an errour of the press. C.

tremity

Rectified once from muriate of lime, it became 0·7864; and this was the lightest I obtained. Mr. Trommsdorf says he has had it at 0·75; but I never found any so light. It is even probable, that what I estimate at 0·7864, having been condensed at a cold of 12° or 15° below 0 [5° or 10·4° F.], is nearly the lightest possible. It burns with a white flame externally, and a fine blue inner flame. It leaves no residuum after combustion. It boils at 59° [138·2° F.]. I do not know at what degree it becomes solid, as I exposed it only to 15° below 0 [5° F.] when it remained perfectly fluid. It mixes with water in all proportions, as well as with alcohol, and all the volatile oils I tried. With olive oil it appears to mix in certain proportions, which vary as one or the other predominates; except heat be applied, when they mix whatever their quantity may be: so that at a temperature much below its boiling point the pyroacetic spirit has the remarkable property of mixing with water, with alcohol, with fixed oils, and with volatile oils, in all proportions. When cold it dissolves a very little sulphur, and a little more phosphorus. Camphor has not a more active menstruum. White wax, as well as grease, dissolves in it hot; but part of both separates, when the temperature is lowered. Water however occasions a copious precipitate from both solutions when cold. It dissolves a little elastic resin, which water precipitates from it. An aqueous solution of gum arabic is rendered very turbid by it.

Taught by the labours of Mr. Thenard, that acetic ether is nothing but a combination of acetic acid with alcohol, and that this combination yielded to the action of alkalis, I was willing to try the pyroacetic spirit comparatively with acetic ether. Acetic ether decomposable by alkalis.

Into acetic ether of the specific gravity of 0·8627 I put some caustic potash, which dissolved in it, and the liquid became yellow. Soon after, and in proportion as the potash dissolved, it lost the smell of ether. Subjecting it to distillation, the solution grew deeper coloured by concentration, and a weak alcohol of the specific gravity of 0·9059 passed into the matrass. In the retort Experiment.

I found

I found acetate of potash, with some potash not saturated.

Pyroacetic spirit treated in the same way.

Into an equal quantity of pyroacetic spirit of the specific gravity of 0.8086 I put a great deal more caustic potash, which dissolved in it slowly, and the liquor became of a very bright yellow. I left it thus at least a fortnight. The whole of the potash was dissolved, the liquor was grown much deeper coloured, and its smell was more fragrant, though not fundamentally changed. On distilling this liquor, it came over very clear and colourless, with the same smell, and all its other characteristics.

Farther comparative experiments.

On preparing the potash in Mr. Berthollet's manner, the alcoholic solution became browner in proportion to its being concentrated; and at length carbon was formed, which floated on a very clear and colourless fluid, but at length disappeared. The solution of potash in pyroacetic spirit never lost its colour; but, on distilling it to dryness, a brown mass remained in the retort. This brown mass I dissolved in water, and again reduced to dryness. It was brown and shining. Exposed to the air for eight and forty hours in a platina capsule it did not perceptibly attract moisture; its taste was a little saponaceous and acrid; and acids threw down from it a yellow flocculent precipitate.

I dissolved a fresh quantity of potash, in the liquor that I had already treated with this alkali, and distilled it afresh in the manner described. The phenomena were precisely the same. Thus it appears, that the potash exerts its action at the expence of the entire substance of the pyroacetic spirit, and not on any oil, or other matter, contained in it.

Action of acids tried.

I tried the action of sulphuric, nitric, and muriatic acid on the pyroacetic spirit, to learn whether it were capable, like alcohol, of forming an ether.

Pyroacetic spirit with sulphuric acid,

Into two measures of the spirit, of the specific gravity of 0.8086, I poured one of moderately concentrated sulphuric acid. The mixture heated a little. It became brown immediately, and a little after very black and thick. After leaving it thus for a fortnight, I distilled it. Two fluids passed over: one colourless, and heavier: the other yellow, in smaller quantity, and floating on the former. They had a strong

a strong smell of sulphurous acid. A large quantity of coal remained in the retort. The first mixture, as well as the residuum of the distillation, was blacker, and the coal more abundant, then when alcohol is treated in the same manner.

Two measures of the same pyroacetic spirit, and one measure of concentrated nitric acid, assumed a very bright and fine yellow colour, like a solution of gold, but grew darker in the course of a fortnight. I afterward distilled this mixture. At the bottom of the liquor was formed a drop of yellow oil, having the appearance of phosphorus melted in water. Nitrous gas was evolved, and this drop disappeared. A liquid passed over, that had a strong smell of nitric acid. This product I saturated with potash, and the spirituous liquor I separated by distillation. It had peculiar characters, which the smallness of its quantity did not allow me to examine minutely. In the residuum I found nitrate and acetate of potash. Oxalic acid may be obtained from the carbonaceous matter, that remains in the retort after the first distillation of pyroacetic spirit with nitric acid. nitric acid,

I distilled one measure of pyroacetic spirit with two of fuming muriatic acid. The liquor in the retort became brown; and, as it passed over, it was wholly condensed in the receiver. It had a strong smell of muriatic acid, and reddened blue vegetable colours. I redistilled it from carbonate of potash: when it acquired a strong smell of turpentine, and a sharp and oily taste. It was far less volatile than muriatic ether. Potash discovered no muriatic acid in it; but when burned on a solution of nitrate of silver, a very copious precipitation of muriate of silver was produced. muriatic acid,

To form this combination in a more advantageous manner, I passed a stream of muriatic acid gas through pyroacetic spirit for a considerable time. It became very brown. I distilled it from carbonate of potash, and two fluids passed over; one perfectly clear and colourless; the other lighter, with a tinge of yellow. The smell of the latter resembled that of pyroacetic spirit, but it was more fragrant. It had a hotter and more oily taste. It mixes but in very small and muriatic acid gas.

quantity with pyroacetic spirit. It requires about forty parts of water to dissolve it. It shows no mark of acidity, and no test discovers the presence of muriatic acid in it; yet, by burning it on a solution of nitrate of silver, a very copious precipitate of muriate of silver is formed.

The spirit compared with alcohol and essential oil.

Alcohol, pyroacetic spirit, and oil of turpentine, have the property of forming combinations with muriatic acid, but each gives a result of a different nature. That of the pyroacetic spirit with this acid is neither an ether, nor a substance resembling camphor.

Is it a simple or compound product?

But are we to consider the pyroacetic spirit as a simple vegetable product, with respect to its immediate composition, as common alcohol is for instance? or as a combination of a vegetable substance with some other matter, as certain ethers, or a solution of oil in ardent spirit? Destructive distillation cannot give us certainty on this head; since from a mixture of all the substances of the vegetable kingdom we ultimately derive the same products in this way. I have made a great many experiments, in order to resolve it into other immediate principles; but hitherto it has appeared to me simple, in the sense commonly given to this word with regard to the vegetable kingdom.

Farther examination for prussic acid.

The smell of bitter almonds, which, in combination with others, characterises this substance, led me to suspect in it the presence of prussic acid; but as analysis failed to detect it, I had recourse to synthetical means.

Prussic acid combined with alcohol.

I passed a stream of prussic acid gas into alcohol, till it was supersaturated, and then distilled the mixture. It retained a strong smell of prussic acid. A second time I distilled it from carbonate of potash. The smell was nearly the same. I added carbonate of potash with black oxide of iron, and, at the expiration of a few days, distilled again. The smell of prussic acid was a little diminished. I then dissolved caustic potash in it, and distilled again. The smell of the liquor condensed in the receiver was still that of prussic acid, but somewhat modified, and participated a little in that of animal matter. The taste was sweet, but pungent. Its specific gravity was 0.8228.....

Its

Its properties differed from those of pyroacetic spirit; and convinced me, that prussic acid combines with alcohol.

The pyroacetic spirit has some properties, that distinguish it from alcohol, ethers, and volatile oils; and others common to it and these substances. It cannot be classed absolutely with either, though it is in some respects related to each. Pyroacetic spirit related to alcohol, ethers, and essential oils.

From the action of potash and acids, it would seem, that a larger proportion of carbon in its ultimate composition distinguishes it chiefly from alcohol. Contains more carbon than alcohol.

A complete analysis of this substance is wanting, to make us better acquainted with it. This I am proceeding to attempt, in order to complete the series of researches, which I proposed to myself for ascertaining its intimate nature.

IV.

On raising Grass Seeds, and preparing Meadow Land: by Mr. WILLIAM SALISBURY, *of the Botanic Gardens, Brompton and Sloane Street *.*

SIR,

I OBSERVE in a list of premiums offered by the Society of Arts, &c., which fell into my hands, that the subject of select grass seeds has engaged their attention. I have, therefore, taken the liberty of sending herewith a sample of festuca pratensis, or meadow fescue grass, which has been grown under my directions; it is a sample of upwards of twenty quarters, the produce of twelve acres of land, which have now been under that crop for the three last years. Grass seeds cultivated to a considerable extent.

As the cultivation of grasses, in general, has engaged my attention very particularly for the last twenty years, I have The poa not adapted to general cultivation.

* Trans. of the Society of Arts, vol. xxvii, p. 67. The silver medal was voted to Mr. Salisbury for this communication.

had an opportunity of observing more attentively the several qualities, and must beg leave to observe, that the opinion formed by Mr. Curtis and others of the two species of *poa* have been proved to be erroneous in several respects; and which it will be highly proper hereafter to notice, as I have found, after numerous experiments, that neither of them is likely ever to be brought into general cultivation, for several reasons which I have fully ascertained from facts. If the mode I have practised of managing the *fescue*, *foxtail*, and other grasses, whether with the view of producing seed, or the best mode of using them in forming pasture and meadow-land, be deserving the notice of the Society, I shall be happy at a future period to send it to you.

Meadows of excellent quality speedily formed.

I beg leave to mention, that there are several meadows at Roehampton, the property of the late Benjamin Goldsmid Esq., which have been made at different times under my management with the grass seed I had previously raised, and which will challenge any other grass land in similar situations in the kingdom. The turf and pasturage of them were formed in less time by far than is to be done by the usual modes practised.

I am, most respectfully, Sir,

Your very humble servant,

WILLIAM SALISBURY.

Certificates were received, dated March 5, 1809, from William Underell, Richard Hook, and Thomas Hook, stating, that in the year 1808 there was grown upon Lee place farm, near Godstone, in Surry, in the occupation of Mr. Pennington, one hundred and sixty-five bushels of meadow fescue grass seed, which was produced from about twelve acres of land, and that it was free from mixture of other grasses or weeds, and that they assisted in harvesting and threshing the whole.

Observations

Observations, in Addition to the preceding Statement; on the Method of laying down Pasture and Meadow Land, with an Account of some Pastures made with the Festuca Pratensis, Lin., or Meadow Fescue-Grass, and Clovers, by Mr. WILLIAM SALISBURY.

DEAR SIR,

IT is now nearly thirty years since my much respected friend and partner, Mr. Curtis, wrote his observations on several of our native grasses, better adapted to the purposes of pasture than ray-grass, the only species of these various tribes that was then, or even now, generally used for the purpose; yet it is acknowledged, by all persons conversant on this subject, that it is inferior to many others, both in produce and nutriment, and also that it remains in the ground but a short time, a fact which attaches also to all the clovers, and is to be lamented by the grazier. Ray-grass inferior to many other kinds.

I have been often more mortified than surprised to find, that, after so much has been written on this interesting subject, and when other grass seeds may be obtained, almost every person is of opinion, that they cannot be cultivated to advantage. I am ready to admit much truth in this, when they are sown according to the common system in practice, which I have frequently convinced myself and others to be erroneous, and that it prevents these useful plants from succeeding after the seeds have been sown. Others not cultivated to advantage from erroneous practice,

It will readily be supposed, that I allude to the mode of laying down land to grass under a crop of spring corn; and I am fully sensible, that many persons will say, that it would be madness to sacrifice the benefit of a crop of barley or oats, where the land is in fine order, and while we can have a crop of grass under it. in laying land down to grass under a crop of spring corn.

To this I reply, that there is no land whatever, when left for a few months in a state of rest, but will produce naturally some kind of herbage, good and bad; and thus we find the industry of man excited, and the application of

the

the hoe and the weeder continually among all our crops, this being essential to their welfare. I cannot help, therefore, observing, how extremely absurd it is to endeavour to form clean and good pasturage under a crop, that gives as much protection to every noxious weed as to the young grass itself*. Weeds are of two descriptions, and each require a very different mode of extermination. Thus if annual, as the charlock and poppy, they will flower among the corn, and the seeds will ripen and drop before harvest, and be ready to vegetate as soon as the corn is removed; and if perennial, as thistles, docks, couch-grass, and a long tribe of others in this way, well known to the farmer, they will be found to take such firm possession of the ground, that they will not be got rid of without great trouble and expense.

Extermination of weeds.

Advantage of the corn crop overbalanced.

Although the crop of corn thus obtained is valuable, yet, when a good and permanent meadow is wanted, and when all the strength of the land is required to nurture the young grass thus robbed and injured, the proprietor is often at considerable expense the second year for manure; which, taking into consideration the trouble and disadvantage attending it, more than counterbalances the profit of the corn crop.

Formation of permanent meadows.

To accomplish fully the formation of permanent meadows three things are necessary, namely, to clean the land, to procure good and perfect seeds adapted to the nature of the soil, and to keep the crop clean by eradicating all the weeds, till the grases have grown sufficiently to prevent the introduction of other plants. The first of these matters is known to every good farmer,—the second may be obtained,—and the third may be accomplished by practising the modes in which

* I do not wish it to be understood, that I allude to the system, practised in Surry and other counties, of sowing clover and rye-grass under barley, when it is intended only for a season or two, in order to change the course of crop, and to be returned again to arable crops as soon as the clover is exhausted, or, what is more frequently the case, overpowered with the weeds that have been nurtured with it; this is totally different, and is not intended for a permanent crop.

which I have succeeded at a small comparative expense and trouble, and which is instanced in a meadow immediately fronting Brompton Crescent, the property of Angus Macdonald Esq., which land was very greatly encumbered with noxious weeds of all kinds, but, by the following plan, the grasses were encouraged to grow up to the exclusion of all other plants, and, though it has been laid down six years, the pasturage is now at least equal to any in the county.

Method of laying down land in grass.

Grass seeds may be sown with equal advantage both in spring and autumn; the land above-mentioned was sown in the latter end of August, and the seed made use of was one bushel of meadow fescue, and one of meadow-foxtail grass, with a mixture of fifteen pounds of white clover and trefoil; the land was previously cleaned as far as possible with the plough and harrows, and the seeds sown and covered in the usual way. In the month of October following, a prodigious crop of annual weeds of many kinds had grown up, were in bloom, and covered the ground and the sown grasses; the whole was then mowed and carried off the land, and by this management all the annual weeds were at once destroyed, as they will not spring again if cut down when in bloom. Thus while the stalks and roots of the annual weeds were decaying, the sown grasses were getting strength during the fine weather; and what few perennial weeds were amongst them were pulled up by hand in their young state. The whole land was repeatedly rolled to prevent the worms and frost from throwing the plants out of the ground; and in the following spring it was grazed till the latter end of March, when it was left for hay, and has ever since continued a good field of grass.

Grass seeds sown in the spring,

The meadows at Roehampton, belonging to the late B. Goldsmid, Esq., were laid down with two bushels of meadow fescue-grass, and fifteen pounds of mixed clover, and sown in the spring along with one peck and a half of barley, intended as a shade to the young grasses; the crop was thus suffered to grow till the latter end of June, and then the corn, with the weeds, were mowed and carried off the land; the ground was then rolled, and at the end of July the

grasses

grasses were so much grown as to admit good grazing for sheep, which were kept thereon for several weeks. It should be observed, that the corn is to be mowed while in bloom, and when there is an appearance of, or immediately after rain, which will be an advantage to the grasses, and occasion them to thrive greatly.

and autumn.

I sowed some fields for the same gentleman in autumn in the same way, and found them to succeed equally well.

A history of plants used in agriculture preparing.

I intended to have made some remarks on some properties, which had escaped Mr. Curtis's notice in his observations, and which do not add to the celebrity of all the grasses he has mentioned; but as I have partly prepared a short history of the nature of all our plants used in agriculture, so far as relates to their properties in a wild state, and the effect of cultivation upon them, I shall, for the present, defer any farther remarks thereon.

I remain, dear Sir,

Your very obedient servant,

WILLIAM SALISBURY.

V.

On a Method of examining the Divisions of astronomical Instruments. By the Rev. William Lax, *A. M. F. R. S.* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. Astronomer Royal* *.

Dear Sir,

St. Ibbs, Aug. 27, 1808.

Importance of some mode of ascertaining the accuracy of an instrument.

I AM persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, the exactness of which you have no adequate means of ascertaining, but are obliged to depend for it in a great mea-

* Philos. Transact. for 1809, p. 232.

sure

sure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read off with so much precision, if we are still uncertain, whether there may not be an errour in the instrument itself of much greater magnitude, than those which we are endeavouring to prevent; and that our best instruments must be liable to such errours, no person can possibly doubt, who has paid due attention to the sources whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in Bird's method of dividing by continual bisections; and have satisfied myself, that they are much more considerable, than is generally apprehended: but as I cannot obtain such precise information as I could wish, respecting the exactness with which a bisection can be performed, or a length taken from the scale of equal parts and laid upon the instrument, I will not trouble you with the deduction which I have made. It is understood indeed, that Bird's method is now generally laid aside, and that each artist employs one, which he considers in many respects as peculiar to himself: but I presumed, that there would still be such a connection betwixt Bird's method and those which have been substituted in its stead, as to render them in some degree liable to the same errours to which it was subject; and the reports which I have uniformly received from persons, who have had an opportunity of examining some of the modern instruments, have fully convinced me, that my opinion was just. But whatever may be the nature of the methods which are now in use, or whatever their advantages over Bird's, I never could persuade myself, that it would be safe to trust to an instrument, without a previous examination. To discover the means of accomplishing this object, is what I have for some time been anxious to effect, and though I fear my endeavours have not been very successful, I will nevertheless take the liberty of presenting you with the result.

Errours in Bird's method may accumulate more than is generally supposed.

Other methods in some degree liable to the same.

You are aware, I believe, that I use a circular instrument for observing both in altitude and azimuth, which was made for me by Mr. Cary in the Strand; that the radius of both the altitude and the azimuth circle is one foot, and that each is divided into parts containing ten minutes. The construction

Altitude and azimuth circle of one foot radius.

struction of this instrument does not differ materially from that of other similar instruments, with which you are well acquainted, and I shall not therefore waste your time by giving you a particular description of it. For the purpose of examining the divisions upon the two circles, I procured an apparatus to be prepared by Mr. Cary, which will be very easily explained. To the face of the rim which surrounds the azimuth circle, and with its left end close to the stand which supports the micrometer on the east side, an arc of brass, concentric with the circle itself, and a little more than 90° in length, an inch in breadth, and one eighth of an inch in thickness, is firmly fixed by screws, so as to have the plane parallel to the plane of the circle, and a small portion of its lower surface resting upon the extreme part of the rim. The screws pass through a brass arc, which is fastened to this at right angles, and lies with its broad side against the face of the rim. Upon the first mentioned arc, a strong upright piece of brass, about six inches in length, is made to slide, the lower part of it embracing the arc as a groove, and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a movable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer, as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire, are determined.

Apparatus for examining the divisions.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the

plane

plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that while the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a movable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it, and through the lower micrometer at the same time.

Method of examining the divisions by it.

I will now proceed to show you, in what manner the examination of the divisions upon either circle may be performed. The process is precisely the same in both cases, and will of course be described in the same words.

The first point to be examined is that of 180°, which must be done in the usual way, by bringing the points of 0 and 180° to the movable wires of the opposite micrometers; then turning the circle half-way round, and bisecting the points again with the movable wires; and lastly, taking half the difference betwixt the distances of the wires in the two positions of the circle for the errour at the point of 180°. Having now bisected the point of *zero* with the movable wire of the micrometer, which is intended to be used in the rest of the process (for we shall have no farther occasion for both), we must slide the microscope along the arc, till by moving the wire a little we can bisect the point of 90°, and then the micrometer must be firmly clamped to the arc. The circle must then be turned till the point of 180° is brought to the microscope, and that of 90° to the micrometer, so that we may be able to bisect each by a slight motion

Method of examining the divisions by it.

motion of their respective wires. This being done, we must observe, from the positions of the wires, how much the interval betwixt them has increased or decreased in the measurement of the new arc: and this increase or decrease must be noted down with a + or − accordingly. In the same manner we must proceed through the remaining two arcs of 90°, observing and noting down the difference betwixt each and the original arc.

The point of *zero* must now be brought again to the micrometer, and bisected by the movable wire, and the microscope be made to slide back along the arc, till by moving the wire a little we can bisect the point of 60°, and when this is done, the microscope must be clamped. We must then measure the arc of 60° against every succeeding arc of 60° in the circle, precisely in the same way that we measured the first arc of 90° against the other three. The arc of 45° is next to be measured against every succeeding arc of 45°, and this will complete all that is necessary to be done in the early part of the morning before the heat of the sun can have affected the temperature of the instrument. The rest may be performed at our leisure.

You will immediately perceive the object of this kind of measurement. It enables us to determine, with any degree of accuracy that may be required, the proportion which the first and every succeeding arc of the circle, contained betwixt the micrometer and the microscope, bears to the whole circle, and of course the absolute length of the arcs themselves. Let a denote the real length of the first of these, and $\pm a'$, $\pm a''$, $\pm a'''$, &c., the difference betwixt the first and second, the first and the third, &c., respectively: let A represent any other arc, the length of which is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') + \&c. \ldots (\overline{a + a'''\cdots}^{\,n-1}) = A$, and $a = \dfrac{A - a' - a'' - a''' \cdots \overline{a'''\cdots}^{\,n-1}}{n}$. Hence it is evident, that, if there is no errour committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a'$, $a + a''$, $a + a'''$, &c., and of any arc, comprehending

Method of examining the divisions by it.

prehending any number of these, accurately determined. But if there be an errour of e in the measurement of the first, e', e'', e''', &c., in the measurement of the second, third, &c., respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e'') + \&c. \dots (a + a'''\dots\overline{n-1} + e + e'''\dots\overline{n-1}) = A$, and consequently a will appear to be equal to

$$\frac{A - a' - a'' - \dots a'''\dots\overline{n-1}}{n} - \frac{\overline{n-1}\; e - e' - e'' - \dots e'''\dots\overline{n-1}}{n},$$

which differs from its true value by $\frac{\overline{n-1}\,e + e' + e'' + \dots e'''\dots\overline{n-1}}{n}$. Hence it follows, that the value of the p^{th} arc (p being greater than unity,) as deduced by this process, will differ from its true value by $\frac{\overline{n-1}.e + e' + e'' + \dots e'''\dots\overline{p-1} + e'''\dots p + \dots e'''\dots\overline{n-1}}{n} - e - e'''\dots\overline{p-1}$; and that, if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an errour in this value (and the expression holds when p is unity, or the first arc only is taken) equal to

$$p\,\frac{\overline{n-1}\,.\,e + e' + e'' + \dots e'''\dots\overline{p-1} + e'''\dots p + \dots e'''\dots\overline{n-1}}{n} - \overline{p-1}\cdot e - e' - e'' - \dots e'''\dots\overline{p-1} = \frac{\overline{n-p}\cdot e - e' - e'' - \dots e'''\dots\overline{p-1} + p\,.\,e'''\dots p + e'''\dots\overline{p+1} + \dots e'''\dots\overline{n-1}}{n}.$$

Now, if we suppose e to be the greatest errour to which we are liable in the measurement of any arc, and each of the succeeding errours to be equal to it, and likewise that e', e'',...$e'''\dots\overline{p-1}$ are all negative; then it will appear, that $\frac{n-p}{n} \times 2pe$ will be the greatest errour that can be committed in determining the value of any arc, by adding together the values of the (p) smaller arcs of which it is compounded. For instance, if the interval betwixt the micrometer

Method of examining the divisions by it.

micrometer and the microscope comprehends an arc of 60°, as marked upon the instrument, and this arc is measured against every succeeding arc of 60° in the whole circle, we shall have the greatest errour that can be committed in deducing the arc of 120° from the addition of the two first arcs of 60°, equal to $\frac{6-2}{6} \times 2 \times 2e = 2.66e$. After these remarks, we may proceed to consider how the remaining divisions upon the circle may be examined with the least probable errour, and to ascertain the amount of the greatest to which the process can in any case be liable.

Let the arc of 30° be now measured against every succeeding arc of 30° in the first, third, fourth, and sixth arcs of 60°; and let the length of each be determined from a separate comparison with the arc of 60°, in which it is comprehended, and not from a general comparison with all the four. The arc of 15° must then be measured against every succeeding arc of 15° in the first, third, fourth, sixth, seventh, ninth, tenth, and twelfth arcs of 30°, and the value of each deduced from a comparison with the arc of 30, in which it is contained. When this is done, we shall have determined the length of every succeeding arc of 15°, of the first arcs of 30°, 45°, 60°, 75° (= 60° + 15°) 90°, 105° (= 90 + 15°), 120° (= 60 + 60°), 135° (= 90° + 45°), 150° (= 120° + 30°), 165° (= 150° + 15°), and 180° in each semicircle.

We must next measure the arc of 5° against every succeeding arc of 5° in the whole circle, and deduce the values of the first, and of the sum of the first and second, in each succeeding arc of 15° from a comparison with the arc of 15° in which they are contained. We must then proceed to determine the values of the first arc of 3° in each 15°, and of its multiples the arcs of 6°, 9°, and 12°. We must also put down the value of the last arc of 3° in each arc of 15°, and then deduce the values of the first and last arcs of 1° in each arch of 15°, from a comparison with the arc of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first arcs of 1°, 3°, 5°, 6°, 9°, 10°, 12°; and by taking the last arc of one degree, which has likewise been determined, from

from the arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the remaining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°; and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°, and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterward that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc. Method of examining the divisions by it.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second; and the lengths of the first and second arcs of 20′ (the sum of which will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down; and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree will give us the length of the first 50′, and complete the operation.

In order to ascertain the greatest possible errour to which we are liable in the examination, let ε denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this errour may occur at each end Greatest possible errour.

of

of the arc, it is evident, that e in the expression deduced above $\left(\frac{n-p}{n} \times 2pe\right)$ will become $2\,\epsilon$, and the expression itself $\frac{n-p}{n}$ $\times 4pe$. Hence the possible errour will be $\frac{2-1}{2}\,4\,\epsilon = 2\,\epsilon$ at $180°$; $\frac{2\,\epsilon}{2} + \frac{2-1}{2} \times 4\,\epsilon = 3\,\epsilon$ at $90°$; $\frac{2\,\epsilon}{3} + \frac{3-1}{3} \times 4\,\epsilon = 3{\cdot}33\,\epsilon$ at $60°$; $\frac{2}{3} \times 2\,\epsilon + \frac{3-2}{3} \times 4 \times 2\,\epsilon = 4\,\epsilon$ at $120°$. The greatest errour must therefore lie betwixt 90° and 120°, and nearer to the extremity of the latter than the former arc. At 105° it will be $5{\cdot}50\,\epsilon$; at 111° it will be $5{\cdot}50\,\epsilon - \frac{2}{5}\,.\,1{\cdot}5\,\epsilon + \frac{5-2}{5} \times 4 \times 2\,\epsilon = 9{\cdot}70\,\epsilon$; and at 111° 10′ it will be $9{\cdot}70\,\epsilon - \frac{1}{6}\,.\,1{\cdot}04\,\epsilon$ (the excess of the errour at 111° above that at 112°) $+\ 3{\cdot}33\,\epsilon = 12{\cdot}86\,\epsilon$, which will be found to be the greatest errour betwixt 105° and 120°, and of course the greatest in the first semicircle. In the other semicircle, the process being the same, the possible errours must necessarily be the same at the same distances from the first point, reckoning the contrary way upon the circle.

The magnitude of the quantity ϵ will of course vary upon circles of the same radius, according to the excellence of the glass employed, and the accuracy of the examiner's eye. It will seldom, however, exceed one second upon a circle, the radius of which is one foot; and in general it will not amount to so much. I find that I can read off, to a certainty, within less than three fourths of a second, and hence I conclude, that I could examine the divisions of my circle without being liable to a greater errour than 9·63 seconds, and those of a circle of three feet radius without the risk of a greater errour than 3·21 seconds.

This inconsiderable compared with those incident to dividing,

To those people who are accustomed to entertain such exalted notions of the accuracy, with which astronomical instruments can with a certainty be divided, this errour, I dare say, will appear very considerable; but for my part, I am perfectly satisfied, that it bears but a small proportion to the accumulated errour which may take place, in spite of the utmost vigilance of the artist, in an instrument divided according

according to any method which has hitherto been made public. I need not, however, remark upon the very great improbability, that the errour of examination should ever attain, or approach, to its extreme limit, as this must be sufficiently obvious to any person, who is in the least degree conversant with the doctrine of chances; but it may be proper to observe, that we have it in our power (and in this respect the examiner possesses a most important advantage over the divider of an instrument) to diminish its probable amount, as much as we please, by bringing the movable wires of the micrometer and microscope several times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary,) and the mean taken in little more than a minute, so that the time of performing the work will not be so much increased, as might perhaps have been apprehended; and when it is completed, we may reasonably presume, that the distance of every point from zero (while the temperature of the circle continues uniform) will have been determined with sufficient exactness for every practical purpose.

will scarcely ever occur,

and may be greatly diminished.

Of the time necessary for the examination a pretty correct idea may be formed, by considering how many measurements are required, and allowing about a minute and a half for each; *i. e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear, that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

Time required for the examination.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the

It renders great precision in an instrument unnecessary;

the instrument, totally unnecessary. Whoever indeed employs this method of examination will be virtually the divider of his own instrument; and all that he will ask of the artist is, to make him a point about the end of every five or ten minutes, the distance of which from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe, that by this examination we shall not only be secured against the errours of division, but against those which arise from bad centring, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

counteracts bad centring and imperfection of figure;

It will, I dare say, have occurred to you, that, whenever we are desirous that an observation should be particularly exact, we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against the whole circle, or against the multiple of it, which approaches nearest to the circle, and thence to deduce its value in the manner explained above; and we shall either have entirely excluded the errour which we apprehended, or have rendered it too small to be of any importance. Suppose, for instance, that the arc determined by the observation was 48°; then by measuring it against the whole circumference increased by an arc of 24°, we shall obtain a result free from any greater errour of unequal temperature, than one eighth of the increase or decrease of this arc of 24° beyond a due proportion to that of the circle itself.

may be employed to obviate errours from inequality of temperature;

This expedient gives us all the advantages of the French circle of repetition, without the inconvenience arising from being obliged to turn the instrument, and move the telescope, so many times in the course of the observation. Nay, I am persuaded, that the result may be made more accurate in this way, than by the French method; because not only can the object be more frequently observed, but the contacts or bisections, it may be presumed, will be more exact, when the observer is not disturbed by the hurry attendant upon the use of the repeating circle; and with respect to any errour in the instrument, from whatever cause it may arise, it will be as effectually excluded by the process which I re-

gives all the advantages of the French circle of repetition without its inconveniences, and is even superior to it;

I recommend, as by moving the telescope round the circle. Besides, this method is applicable either to the azimuth or altitude circle, or indeed to any circle which turns upon its own axis, whereas the French method can never be applied to the azimuth circle, or to any other circle, which does not turn both upon its own axis, and upon one which is perpendicular to it.

and is applicable to all circles turning upon their own axis, which the French method is not.

After all, however, it is possible, that the process which I have been explaining to you may be no new discovery, and that you may be already acquainted with it. If this should be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, if you would favour me with your sentiments upon the subject, as soon as you can do it with perfect convenience to yourself.

I am, Dear Sir,

Yours, &c.

WILLIAM LAX.

VI.

A Physical View of the Equatorial Regions, drawn up from Measures taken and Observations made on the Spot, from the tenth Degree of North to the tenth of South Latitude, between the Years 1798 *and* 1804, *by Mr.* VON HUMBOLDT *.

THE author has here collected together the natural phenomena of the equinoctical regions, from the level of the Pacific Ocean to the highest peak of the Andes. The particulars, from which these results are given, will be found in his travels at large; but this connected view of their principal features cannot fail to be highly interesting to the natural philosopher. Mr. von Humboldt has not extended this summary nearer the tropics than the latitude of 10°, on account of the great difference observed, not only in the productions of the soil, but more particularly in the meteorological phenomena, between 10° and 23°.

Natural phenomena in the vicinity of the equator.

These vary greatly on approaching the equinox.

* Magazin Encyclopédique, Sept. 1807, p. 139.

From

Plain on the west.

From the western shore of America, in this region, to the chain of the Cordilleras, we find a plain stretching a considerable distance from north to south, but in no place exceeding twenty or thirty leagues in breadth.

Chimborazo.

Chimborazo, the loftiest summit of the vast chain of the Andes, rises in

Its height.

1° 27′ south latitude. From the author's barometrical measurements, calculated by the formula of Mr. Laplace, this mountain is 6536 met. [7143 yards] above the level of the sea. Mr. Condamine gives 180 met. [197 yards] less: a difference naturally arising from his neglecting the temperature.

Vast volcano.

In the map accompanying this volume, where Mr. von H. has given a vertical plan of Chimborazo, the summit of Cotopaxi appears behind it; a volcano nearly five times as high as Vesuvius, and the roarings of which were heard in 1744 to the distance of 220 leagues.

The east face of the Andes steepest.

Several eminent natural philosophers have supposed, that the western slope of the Andes was much steeper than that on the east: but Mr. von Humboldt has found the contrary to be the case, after traversing the chain in several directions.

Geography of plants.

The part which the author has dedicated to the geography of plants exhibits many new views. On the plain of Chimborazo he has inserted the names of the plants growing on it at different heights, which he determined accurately by means of the barometer. His comparison of them with the plants growing at the same heights on the Alps and Pyrenees is extremely curious: and he has noted the height they attain, according to the different situations and elevations where they grow.

Variation of temperature.

This article is followed by a series of observations on every thing that varies with the heights to which you ascend above the level of the sea. The table of variations of temperature exhibits the maximum and minimum of heat, which the centigrade thermometer indicates from five metres to

Cold of the Andes.

five hundred. It appears, that the cold of the Andes is not very considerable; but many circumstances combine to render it difficult to bear. On Chimborazo, at the height of 5908 met. [6457 yards], the thermometer descends only

Heat near the rivers.

to −1·8° [28·76° F.]. In the hottest regions on the banks of the Amazon, Magdalena, and other rivers, the mean

mean temperature is 27° [80·6° F.]; but the thermometer seldom rises to the extremes of heat, which it frequently attains in the north of Europe. In the equatorial regions the extremes of the greatest and least heat are not more than 16° or 20° [28·8° or 36°] distant; while in Europe they exceed 62° [81·6°].

Extremes of heat and cold.

From all the observations made by Mr. von H. on the Cordilleras it appears, that the decrease of temperature above 3500 met. [3825 yards] is more rapid in the proportion of 5 to 3, than from the level of the sea to the height of 2500 met. [2732 yards]: but it is to be remarked, that the inequalities of the surface of the Earth have much influence on this decrement; and that a person going up to the same heights in a balloon would find different results.

The temperature decreases more rapidly at great heights.

This influenced by the inequality of the surface.

Mr. von Humboldt ascended Chimborazo to such a height, that the barometer fell to 0·37717 of a metre [11·81 inches]: its height at the level of the sea being settled at 0·76202 of a met. [29·98 in.], and the temperature being 25° [77° F.]. This differs a little from the estimation of Bouguer, who made it 0·76022 of a met. [29·91 in.].

Heights ascended by the author.

The elasticity of the air in the temperate zones varies sometimes 0·045 of a met. [1·77 in.] in the same place. Under the tropics, at the seaside, it varies only 0·0026 of a met. [1·023 line]. In a separate table Mr. von H. gives these variations at the different hours, at which they are noticed. The progress of these variations under the equator, at the level of the sea, supposing the mean term of the barometer = Z is as follows.

Variations of the barometer.

At 21 hours = Z + 0·5	At 11 hours = Z + 0·1
At 4 = Z − 0·4	At 16 = Z − 0·2

These horary variations are not noticed in our climates, on account of the many local causes, that occasion the barometer to rise or fall irregularly.

Mr. von H. concludes his barometrical researches with some physiological observations on the connexion between respiration and the elasticity of the air. It appears that the inhabitants of the plains, accustomed to a pressure of 28 in. [29·84 Eng.], most easily support these variations of the density of the air: they easily habituate themselves

Effect of the weight of the air on respiration.

 to

to that of Quito, which answers to 20 in. 1 line [21·4 in. Eng.], and other places still higher, where the barometer falls to 17 in. 4 l. [18·47 in. Eng.].

Hygrometrical observations.

In his hygrometrical observations Mr. von H. sometimes used Saussure's hygrometer, sometimes Mr. De Luc's; but all his results were reduced to the degrees of Saussure's, correcting them for temperature.

Moisture on the summit of the Andes.

On the summit of the Andes, where the hygrometer falls to 31·7°, there still remains great moisture; and the freshness retained by the vegetation sufficiently proves, that it derives from aqueous vapour the aliment, that supports it amid extreme drought.

Height of clouds.

The aqueous vapour, which exhibits itself in large masses, seems to maintain nearly a constant height. From the measures of Mr. von H. the lower stratum appears to support itself at 1169 met. [1277 yards], and the higher at 3300 [3606 yards] above the level of the sea. As to those little clouds, which are vulgarly termed flocks [*moutons*], it is very remarkable, that they commonly rise to the height of more than 7800 met. [8524 yards].

Quantity of rain.

Mr. von H. estimates the quantity of rain, that falls annually between the tropics, at more than 1·89 met. [74·36 in.], while in Europe it is only 0·48 [18·88 in.*].

Electricity of the atmosphere.

The electrometrical table exhibits several equally curious results. The electric intensity increases considerably as you approach the summit of the Cordilleras. The equatorial regions from the sea to the height of 2000 met. [2186 yards] are but slightly charged with the electric fluid; it accumulates in the clouds however, and there causes frequent explosions. They recur periodically, in general two hours after the culmination of the sun; and their violence in these climates is well known. At the height of 3000 met. [3278

Hail.

* This is certainly much too small. A mean for 11 different places in England for 1809, see Journal, XXV, p. 309, gives 31·15: this is near Mr. Dalton's mean for all England, taking first a mean of the counties: and a mean of several places on the continent, as well as in England, from the table in Dr. Young's Natural Philosophy, vol. II, p. 477, gives 32·1. C.

yards]

yards] a great deal of hail is formed; and the atmosphere at this height is commonly charged with negative electricity.

It is well known, that the blueness of the sky is deeper in proportion as the air is more dilated. Mr. von H. thought he observed, that in general it was more intense under the tropics, than at an equal height in Europe. This he ascribes to the complete solution of the vapours in the equatorial atmosphere. On the Andes the blueness was 46° of Saussure's cyanometer. Blueness of the sky.

The decrease of light is greater in proportion to the density of the strata of the air; accordingly it is much less on the summits of high mountains. In general the light is much stronger under the tropics, than at equal heights in Europe. This is confirmed by the light reflected from the moon to the Earth in a total eclipse. This intensity of the light probably acts on vegetable productions, and contributes to that resinous and aromatic character, which they exhibit on the tops of mountains. Perhaps too it exerts its influence on the nerves; as the inhabitants of Quito have a sensation of faintness, whenever the sun darts his rays on them. Light.

Bouguer calculated, that the atmosphere, reduced to the temperature of 0° [32° F.] throughout its whole extent, and to a column of mercury of 0 76 met. [29·9 in.], would be 7820 met. [8546 yards, or not quite 5 miles]: the observation of the twilight shows, that at 60000 met. [65571 yards, or rather more than 37 miles] the air has sufficient density to send us a perceptible light. Atmosphere

Mr. von H. gives some results of experiments made by him and Mr. Gay-Lussac on the chemical composition of the atmosphere. From these experiments the component parts of the air are 0·210 of oxigen gas, 0·787 of nitrogen, and 0·003 of carbonic gas. If there be any variation, it appears not to exceed a thousandth part of oxigen; and Mr. Gay-Lussac found it the same at the height of 7000 met. [7650 yards]. Uniformity of its composition.

Of the interesting series of geological observations we can give here but a few particulars.

The

Mountains.

The height of the loftiest mountains is so trifling compared with the radius of the Earth, that it has had very little influence on the grand phenomena of geology: but it is perceptible with regard to a small part of the Earth's surface, and the law according to which the different rock-formations are arranged above the level of the sea. The equatorial regions exhibit both the loftiest summits, and the most extensive plains. We cannot therefore ascribe to the rotary motion of the globe this group of mountains. Besides, the chain of the Andes at its two extremities extends toward the poles as far as 29° or 30° *. This chain is very unequal: in many places it is not above 200 met. [218·57 yards] high; but in four places it attains a colossal height. These are in 17° south latitude; under the equator in the kingdom of Quito; at Mexico, in the latitude of 19° north; and opposite Asia, in 60° north lat. This height varies from 5000 to 6000 met. [5464 to 6557 yards]. The chain of the Andes is not less astonishing for its bulk, than for its height. Near the volcano of Antisana, at the height of 4000 met. [4371 yards], Mr. von H. found a plain twelve leagues in circumference. In some parts this chain is from 40 to 60 leagues broad.

Chimborazo forms one extremity of a colossal group. The chain that extends to the south stretches so far toward the ocean, that the islands near the continent may be considered as fragments of it. On the north the Cordillera separates into three branches. The easternmost forms the group of Santa Martha and Merida. The westernmost, from which we are furnished with platina, forms a group near the city of Mexico, some of the peaks of which, as Popocatepec and Oribaza, exceed 5300 met. [5792 yards]. This chain afterward diminishes considerably, but in the vicinity of Asia it forms a fourth group, in which Mount St. Elias is 5512 met. [6024 yards] high. In these latitudes the Andes appear to have a subterranean communication with the volcanoes of Kamtschatka.

Interior structure of the

In their interior structure the mountains of the equator unite almost every kind of rock discovered in the rest of

* It should be 50° or 60°. C.

the

the globe. The arrangement of their strata too is perfectly similar. Granite forms the base of the chain of the Andes; and their ridge is every where covered with porphyritic formations, basaltes, phonolites, and greenstone. But a circumstance peculiar to the equatorial mountains is the vast height, to which the rocks of subsequent formation to granite ascend. This base is almost wholly covered by these more recent formations, and the highest point at which Mr. von H. observed it was 3500 met. [3825 yards]. The summits of Chimborazo, Antisana, &c., at the height of 6372 met. [6964 yards], are of porphyry. Mr. von H. likewise points out the different heights, at which the other stratifications are met with. The remains of organic bodies are very rare in the mountains near the equator, as calcareous stones are by no means abundant. Some are found however at the height of 4300 met. [4699 yards]. The fossil bones of elephants, which Mr. von H. has collected, were found about 3000 met. [3278 yards] only. equatorial mountains.

The great masses of sulphur, that abound in the Cordillera, are frequently met with in primitive rocks, at a distance from volcanoes. Sulphur.

The greatest number of volcanoes are found in the chain of the Andes. There are more than fifty, between cape Horn and mount St. Elias, that still emit flames. Some of them pour out lava; others, as those of Quito, throw up scorified rocks, water, and clay. Volcanoes.

The most recent volcano is that of Xorullo, or Jorullo, [of which an account has already been given in p. 81 of this volume]. The air collected at the bottom of the crater by Messrs. von H. and Bonpland contained more than 0·05 of carbonic acid.

The lowest limit of the snows under the equator is one of the most constant of the natural phenomena. From the various measures which he took Mr. von H. estimates it at 4795 met. [5240 yards]. The Cordillera of the Andes has no glaciers, owing probably to the little snow, that falls between the tropics; but at Chimborazo, on digging in the earth, snow of very great antiquity is found. Limit of snow.

In his description of the animals that inhabit the Andes, Mr. von H. notes the different heights, at which they are seen.

His

Agriculture.

His last article relates to the cultivation of the soil, in the regions he has sketched. Several villages on the acclivity of the Andes are built at a considerable height. From the level of the sea to the height of 1000 met. [1093 yards] the plantain, maize, and chocolate nut are cultivated. It is the region of the most delicious fruits. The Europeans have introduced other plants; the sugar cane, indigo, and coffee tree. The last of these delights in a high and stony soil.

Wheat grows in the greatest abundance in Quito and Peru from the height of 1600 to 1900 met. [1748 to 2076 yards]. The great plains of the Cordillera are particularly favourable for the corn of Europe. The soil, yielding easily to the plow, shows that they were anciently the bottoms of lakes.

The plains bottoms of old lakes.

From 3000 to 4000 met. [3278 to 4371 yards] the principal article of culture is the potato. Higher up the people live amid their numerous flocks of lamas, which frequently wander as far as the region of snow.

VII.

Observations on the Pleonast Spinel, particularly that of the Environs of Montpellier. By Mr. MARCEL DE SERRES *.

Synonimes of the pleonast, found in Ceylon,

THE pleonast of Hauy, brown garnet or schoerl in truncated dodecaedrons of Romé de Lisle, was originally found in the island of Ceylon, among tourmalines and other crystalline substances, with which it was confounded. Delamétherie first made a distinct species of it under the name of ceylanite. Sometime afterward he found it in rocks thrown up by Vesuvius. Mr. Lhermina subsequently observed it in the same rocks: and Mr. Lewis Cordier lately obtained it from almost all the volcanic rocks in the environs of Closterlach on the borders of the Rhine. Draparnaud mentions pleonast in the breccia of the little basaltic moun-

and distinguished as a species by Delamétherie.

Occurs in volcanic rocks.

* Journal de Physique, vol. LXVII, p. 26.

tain

tain of Montferrier: in fact it is met with there in a tufa, that has the form of a breccia. I have never yet found it in a breccia, but like those that came from Ceylon, which are most commonly in amorphous or rounded masses, that have experienced a commencement of alteration. Perhaps this word is too strong to mark the state in which this substance is found, for it appears too hard, to alter easily; yet its colour is so dull, that it appears to be altered.

An accidental mixture in breccia.

Apparently the pleonast is a very accidental mixture in the breccia: a few of the crystals, detached from the rocks in which they were contained, have been united to the various surrounding substances by some cement*. Crystallized pleonast rubies are found likewise in the bottom of the gullies at the foot of the little mountain of Montferrier, and almost always on the surface of the detritus of the surrounding substances.

Found in a sand near Montpellier.

A totally different situation, where this substance equally presents itself, and in pretty large quantity, is at Soret, on the left bank of the Lez, about a mile from Montpellier. It is found on the surface of a sand mixed with shells and bowldered quartz. This sand rests on strata of sandstone, and very various and heterogeneous agglomerations of the same nature. Sometimes these strata are covered by others of shelly limestone, filled chiefly with the oyster, cockle, and acorn shell. The oysters frequently contain others, which appear to have grown in their cavity. The strata of sandstone are very irregular, most frequently horizontal, and containing numerous concretions of sandstone, in the shape of pears, apples, and tears, almost always in the same position; which indicates, that these concretions were not formed in the manner of common stalactites, but as the nodules of silex. It appears evident, that the pleonasts occur in them accidentally, and were brought thither by the waters. I have since found some in the volcanic hill of

Sandstone strata.

Formation of breccia and sandstones.

* Secondary calcareous waters appear to have formed this breccia, and to enter into the formation of all the known brecciæ and sandstones. In fact, water loaded with carbonate of lime, flowing from the base of the basaltic prisms, has there deposited the earth with which it was impregnated, agglutinated all the broken pieces of the surrounding rocks, and thus formed a breccia.

Valmahargues,

Valmahargues, 6 kil. [$3\frac{3}{4}$ miles] north of Montpellier; and likewise in a stratum of basaltic tufa 3 kil. [15 furlongs] long, at the bottom of a hill called *lou Hâout*, or *lou Nâout*, near Prades, on the north-east of Montferrier.

Is the pleonast a volcanic production?

A question, by no means uninteresting, that naturally presents itself, is, whether the pleonast ruby be a volcanic product, or not. From its hardness it might be presumed, that it is altogether foreign to the lavas, and formed in the humid way, anterior to its deposition in the strata where it is found. It may be said, that none has yet been seen in the lava of the Vivarais, Auvergne, Etna, the Lipari islands, Iceland, or the Isle of France; but only in the cavities of some rocks of Vesuvius, Somma, Closterlach, and Campania. Hence we have sufficient reason to believe, that it belongs to the primitive rocks, and that to see it intimately united with the tourmalines of Ceylon is sufficient to convince us of this. Brongniart however is of opinion, that this mineral, as well as the telesia, or corundum, belongs to the secondary trap formation. His opinion is founded perhaps upon that of Werner; who judges from the nature of the strata, of which the sand containing corundums appears to be the remains, that those of a hardness much superior to the pleonast must belong to that formation. Thus as basaltes and basaltic tufa are rocks of the secondary trap mountains, and these are met with in the places where the pleonast is found, this opinion appears to have some probability: but as we have not yet any accurate description of the mineralogical situation of the corundums; and as the adamantine spar is found in granite rocks, entering even into their composition in the same manner as feldspar; we may consider the corundum, adamantine spar, and pleonast as belonging exclusively to the primitive formations. This opinion however can rank only as a probability, till we have a precise knowledge of the mineralogical situation of these interesting substances.

Probably it belongs to the primitive rocks,

though supposed by some to be of the secondary formation.

VIII. On

VIII.

On the Nodules of Lava found in the Klingstein of the Rock of Sanadoire *.

THE rock of Sanadoire having been described by various naturalists †, we shall confine ourselves to the mentioning of a fact, which had long escaped the notice of those who visited this singular rock, that of nodules of tumefied lava imbedded in the clinkstone. This fact was first observed by Mr. Lacoste in 1804 ‡; and it has been since noticed by Messrs. Menard and Alluaud in the summer of 1807. Having accompanied these gentlemen in their visits to the Mont-d'Or, we shall point out the road we pursued, and the precise places where this fact occurs.

Nodules of lava in the phonolite of Sanadoire.

1. Proceeding from the lake of Guéri to the rocks of Sanadoire and Thuilière, by the road from Mont-d'Or to Rochefort, and beginning to descend toward the valley, where these two rocks are situate, so as to see their summit; on the left of the road, and not above thirty yards from it, is a small eminence, or inequality of the ground, formed of masses and fragments of phonolite, in which nodules of lava are imbedded.

Places where it occurs.

2. On ascending the great slope at the base of Sanadoire, which is on the west, and directly opposite Thuilière, we find similar nodules in the crumbled materials from above, which are all of phonolite. Some of these nodules scarcely adhere to the mass that includes them, and are separable by a single stroke of a hammer. Others adhere to it more closely, but still do not form one continuous substance with it. The surface of those separated by the hammer is rounded, but irregular, smooth, and covered with a slight shining coat, which appears to be a very thin pellicle of klingstein.

These nodules are of different kinds of lava.

The lava of different kinds.

A. A black, compact lava exhibiting in its fracture a

1st kind.

* Journal de Physique, vol. lxvii, p. 54.

† Memoirs of the French Academy, for 1771 and 1773; Desmarets, Mémoire sur le Basalte. Journ. de Phys. tome 59, an 1804. Daubuisson. Mémoire sur la Phonolithe.

‡ Lacoste's Geological Letters, a work in which they who visit Auvergne will meet with facts no where else described.

multitude

multitude of little needles of amphibole, or pyroxene; so that they might be taken for fragments of a hornblende rock, if some of them did not exhibit unequivocal marks of the action of fire.

2nd kind. *B.* A porous, tumefied lava, of a gray and blackish gray ground, with needles of pyroxene, and laminæ of feldspar. This is the most common variety.

Not peculiar to this rock. 3. This fact is not peculiar to the rock Sanadoire and those in its vicinity: a large bowldered block in the torrent of Prent-Garde, coming from the lake of Guéri, exhibited to us the same phenomenon. The klingstein is a variety of that of Sanadoire, of a blueish gray colour, and less polyedral in its fracture.

3d kind. *C.* The lava it contains is gray, and composed of very small crystals mingled together.

Found in another mountain, 4. We could not visit la Védrine, another phonolitic mountain on the east of Mont-d'Or, but Mr. Menard informs us, that he found there the same peculiarities as at Sanadoire, which had wholly escaped his notice at his first visit to that mountain.

and probably in several places. It is not in the simple clinkstone, that we found these nodules of lava; perhaps they would equally be found in the porphyry with base of clinkstone of the environs of lake Guéri, Puy Gros, the rock of Dardanche, &c. By an attentive research, employing the time requisite for an examination of this interesting part of the Monts-d'Or, we have no doubt they would be found in much greater number. One single specimen has presented us with two or three nodules. Their size varies from a few cubic millimetres to fifteen cubic centimetres [from one or two cubic lines to near a cubic inch.]

Size of the nodules.

This stone therefore a lava. This stone has always been considered as a lava in France, either under the name of greenish petrosiliceous lava, or of prismatic and tabular greenish basaltes: but its being found among volcanic substances was the sole ground of this opinion, for marks of the action of fire had no where been observed on it. The volcanic origin, therefore of the rock Sanadoire, long disputed, is now evidently proved by the presence of the nodules of lava it contains, which presupposes the rock to have been in a state of fluidity.

IX.

IX.

Chemical Examination of a Substance found in Balsam of Mecca: by Mr. VAUQUELIN *.

MR. HALLE, a member of the Institute, &c., requested me to examine a substance, which he obtained from balsam of Mecca on dissolving it in alcohol by means of heat. This substance had the appearance of a resin, was transparent, and had an agreeable smell. Thrown on burning coals, it emitted a smoke and smell like those of frankincense, and left no coally residuum.

Portion of balsam of Mecca not taken up by alcohol.

I took a gramme [15½ grs.] of it, and treated it with alcohol at 40°. The first effect of this was to render it opake from the surface to the centre, as soon as it began to exert its action, and it soon gave it a flocculent form throughout. I continued to boil fresh quantities of alcohol on this flocculent substance, till it no longer rendered distilled water turbid.

Boiled repeatedly in alcohol.

The insoluble flocculent matter, being collected, weighed 0·3 of a gramme, or nearly a third of the substance employed. Heat united it into masses of great tenacity, which drew out into threads like birdlime, without however possessing any elasticity. Thrown on burning coals it emitted the same smell of frankincense as before, and left no residuum.

⅓ insoluble.

The alcoholic solution was transparent while hot; but on cooling grew turbid, though it let fall no sediment. During evaporation a white flocculent matter appeared, in proportion as the alcohol was diminished; and when completely dried part remained in a pulverulent and spongy form. Another portion united into a transparent mass, having nearly the appearance of turpentine. Both of these burned like resins, but with a less agreeable smell than the part insoluble in alcohol.

The solution evaporated.

These experiments prove, that in the residuum of balsam of Mecca insoluble in cold alcohol, put into my hands by Mr. Hallé, there are two substances; one, which dissolves in a very large quantity of boiling alcohol; and another,

It consists therefore of two substances,

* Annales de Chimie, vol. lxix, p. 221.

which

which does not combine with it at all, though of a resinous nature.

either employed to sophisticate it, or natural to it;

Do these two substances exist at the same time with the balsam of Mecca in the tree that furnishes it, the amyris opobalsamum? are they formed at the expense of the balsam, by a change effected by keeping? or are they fraudulently mixed with the balsam?

To solve these questions it would be necessary, to examine genuine balsam of Mecca, both when fresh, and after it is old. If the resinous substances abovementioned were nöt found in either, there would be reason to ascribe them to fraud: but it is probable, that they are natural to the balsam of Mecca, since Mr. Halle, who has had frequent occasions of dissolving this balsam in alcohol, alwäys obtained the same result.

but probably the latter.

X.

Of the Fettstein: by Count Dunin Borkowski *.

Character of the fettstein.

THE colour of this stone is sometimes seagreen, sometimes bluish. It is likewise found of a deep fleshcoloured red.

Externally it has but little lustre: internally it is very shining, with a greasy lustre. From this appearance its name, which signifies *fat stone*, is derived.

Its fracture is lamellar; not very determinate, though a double cleavage, with oblique junctures is perceivable; and a little scaly.

It is very translucid on the edges; hard, so as to strike fire with steel; and brittle.

Its specific gravity is 2·563.

Heated before the blowpipe its colour changes to an ashen gray; and it fuses, though with difficulty, into a white enamel.

It is found accompanied with feldspar and amphibole at Arendahl, in Norway.

When the fettstein is cut into a spherical form, it exhibits the same play of colours as the cat's-eye.

* Journal de Physique, vol. lxix, p. 159.

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